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## Ninety-Third Session.

*Monday, 20th December* 1875.

Sir WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

### 1. Vortex Statics. By Sir William Thomson.

(*Abstract.*)

The subject of this paper is *steady motion* of vortices.

1. Extended definition of "steady motion." The motion of any system of solid or fluid or solid and fluid matter is said to be steady when its configuration remains equal and similar, and the velocities of homologous particles equal, however the configuration may move in space, and however distant individual material particles may at one time be from the points homologous to their positions at another time.

2. Examples of steady and not steady motion:—

(1.) A rigid body symmetrical round an axis, set to rotate round any axis through its centre of gravity, and left free, performs steady motion. Not so a body having three unequal principal moments of inertia.

(2.) A rigid body of any shape, in an infinite homogeneous liquid, rotating uniformly round any, always the same, fixed line, and moving uniformly parallel to this line, is a case of steady motion.

(3.) A perforated rigid body in an infinite liquid moving in the

manner of example (2.), and having cyclic irrotational motion of the liquid through its perforations, is a case of steady motion. To this case belongs the irrotational motion of liquid in the neighbourhood of any rotationally moving portion of fluid of the same shape as the solid, provided the distribution of the rotational motion is such that the shape of the portion endowed with it remains unchanged. The object of the present paper is to investigate general conditions for the fulfilment of this proviso; and to investigate, farther, the conditions of stability of distribution of vortex motion satisfying the condition of steadiness.

3. *General synthetical condition for steadiness of vortex motion.*— The change of the fluid's molecular rotation at any point fixed in space must be the same as if for the rotationally moving portion of the fluid were substituted a solid, with the amount and direction of axis of the fluid's actual molecular rotation inscribed or marked at every point of it, and the whole solid, carrying these inscriptions with it, were compelled to move in some manner answering to the description of example (2). If at any instant the distribution of molecular rotation* through the fluid, and corresponding distribution of fluid velocity, are such as to fulfil this condition, it will be fulfilled through all time.

4. *General analytical condition for steadiness of vortex motion.*— If, with (§ 24, below) vorticity and "impulse," given, the kinetic energy is a maximum or a minimum, it is obvious that the motion is not only steady, but stable. If, with same conditions, the energy is a maximum-minimum, the motion is clearly steady, but it may be either unstable or stable.

5. The simple circular Helmholtz ring is a case of stable steady motion, with energy maximum-minimum for given vorticity and given impulse. A circular vortex ring, with an inner irrotational annular core, surrounded by a rotationally moving annular shell (or endless tube), with irrotational circulation outside all, is a case of motion which is steady, if the outer and inner contours of the

* One of the Helmholtz's now well-known fundamental theorems shows that, *from the molecular rotation at every point of an infinite fluid* the velocity at every point is determinate, being expressed synthetically by the same formulæ as those for finding the "magnetic resultant force" of a pure electro-magnet. —*Thomson's Reprint of Papers on Electrostatics and Magnetism.*

section of the rotational shell are properly shaped, but certainly unstable if the shell be too thin. In this case also the energy is maximum-minimum for given vorticity and given impulse.

6. In these examples of steady motion, the "resultant impulse" (V. M.* § 8) is a simple impulsive force, without couple; the corresponding rigid body of example 3 is a circular toroid, and its motion is purely translational and parallel to the axis of the toroid.

5. We have also exceedingly interesting cases of steady motion in which the impulse is such that, if applied to a rigid body, it would be reducible, according to Poinsot's method, to an impulsive force in a determinate line, *and a couple with this line for axis.* To this category belong certain distributions of vorticity giving longitudinal vibrations, with thickenings and thinnings of the core travelling as waves in one direction or the other round a vortex ring, which will be investigated in a future communication to the Royal Society. In all such cases, the corresponding rigid body of § 2 example (2) has both rotational and translational motion.

7. To find illustrations, suppose, first, the vorticity (defined below, § 24) and the force resultant of the impulse to be (according to the conditions explained below, § 29) such that the cross section is small in comparison with the aperture. Take a ring of flexible wire (a piece of very stout lead wire with its ends soldered together answers well), bend it into an oval form, and then give it a right-handed twist round the long axis of the oval, so that the curve comes to be not in one plane (fig. 1). A properly-shaped twisted ellipse of this kind [a shape perfectly determinate when the vorticity, the force resultant of the impulse, and the rotational moment of the impulse (V. M. § 6), are all given] is the figure of the core in what we may call the first† steady mode of single and simple toroidal

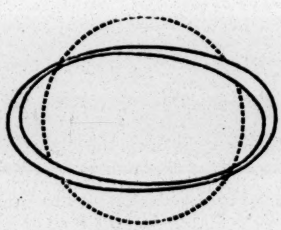

Fig. 1.

* My first series of papers on vortex motion in the "Transactions of the Royal Society of Edinburgh," will be thus referred to henceforth.

† First or gravest, and second, and third, and higher modes of steady motion to be regarded as analogous to the first, second, third, and higher fundamental modes of an elastic vibrator, or of a stretched cord, or of steady undulatory motion in an endless uniform canal, or in an endless chain of mutually repulsive links.

vortex motion with rotational moment. To illustrate the second steady mode, commence with a circular ring of flexible wire, and pull it out at three points, 120° from one another, so as to make it into as it were an equilateral triangle with rounded corners. Give now a right-handed twist, round the radius to each corner, to the plane of the curve at and near the corner; and, keeping the character of the twist thus given to the wire, bend it into a certain determinate shape proper for the data of the vortex motion. This is the shape of the vortex core in the second steady mode of single and simple toroidal vortex motion with rotational moment. The third is to be similarly arrived at, by twisting the corners of a square having rounded corners; the fourth, by twisting the corners of a regular pentagon having rounded corners; the fifth, by twisting the corners of a hexagon, and so on.

In each of the annexed diagrams of toroidal helixes a circle is introduced to guide the judgment as to the relief above and depression below the plane of the diagram which the curve represented in each case must be imagined to have. The circle may be imagined in each case to be the circular axis of a toroidal core on which the helix may be supposed to be wound.

To avoid circumlocution, I have said, "give a right-handed twist" in each case. The result in each case, as in fig. 1, illustrates a vortex motion for which the corresponding rigid body describes left-handed helixes, by all its particles, round the central axis of the motion. If now, instead of right-handed twists to the plane of the oval, or the corners of the triangle, square, pentagon, &c., we give left-handed twists, as in figs. 2, 3, 4, the result in each case will be a vortex motion for which the corresponding rigid body describes right-handed helixes. It depends, of course, on the relation between the directions of the force resultant and couple resultant of the impulse, with no ambiguity in any case, whether the twists in the forms, and in the lines of motion of the corresponding rigid body, will be right-handed or left-handed.

8. In each of these modes of motion the energy is a maximum-minimum for given force resultant and given couple resultant of impulse. The modes successively described above are successive solutions of the maximum-minimum problem of § 4; a determinate problem with the multiple solutions indicated above, but no other

solution, when the vorticity is given in a single simple ring of the liquid.

9. The problem of steady motion, for the case of a vortex line with infinitely thin core, bears a close analogy to the following purely geometrical problem :—

Find the curve whose length shall be a minimum with given

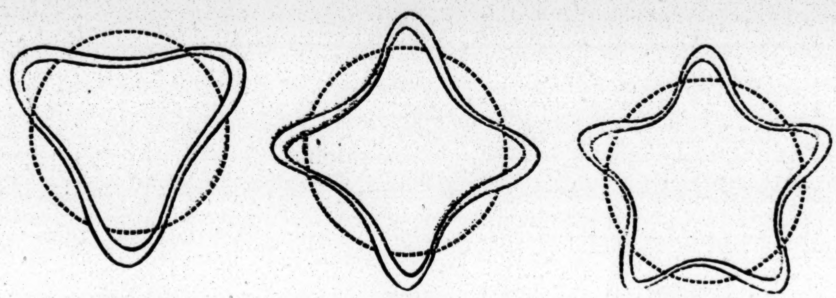

Fig. 2. Fig. 3. Fig. 4.

resultant projectional area, and given resultant areal moment (§ 27 below). This would be identical with the vortex problem if the energy of an infinitely thin vortex ring of given volume and given cyclic constant were a function simply of its apertural circumference. The geometrical problem clearly has multiple solutions answering precisely to the solutions of the vortex problem.

10. The very high modes of solution are clearly very nearly identical for the two problems (infinitely high modes identical), and are found thus :—

Take the solution derived in the manner explained above, from a regular polygon of N sides, when N is a very great number. It is obvious that either problem must lead to a form of curve like that of a long regular spiral spring of the ordinary kind bent round till its two ends meet, and then having its ends properly cut and joined so as to give a continuous endless helix with axis a circle (instead of the ordinary straight line-axis), and N turns of the spiral round its circular axis. This curve I call a toroidal helix, because it lies on a toroid * just as the common regular helix lies

* I call a circular toroid a simple ring generated by the revolution of any singly-circumferential closed plane curve round any axis in its plane not cutting it. A "tore," following French usage, is a ring generated by the revolution of a circle round any line in its plane not cutting it. Any simple

on a circular cylinder. Let $a$ be the radius of the circle thus formed by the axis of the closed helix; let $r$ denote the radius of the cross section of the ideal toroid on the surface of which the helix lies, supposed small in comparison with $a$; and let $\theta$ denote the inclination of the helix to the normal section of the toroid. We have

$$\tan\theta = \frac{2\pi a}{\mathrm{N}.2\pi r} = \frac{a}{\mathrm{N}r};$$

because $\frac{2\pi a}{\mathrm{N}}$ is as it were the step of the screw, and $2\pi r$ is the circumference of the cylindrical core on which any short part of it may be approximately supposed to be wound.

Let $\kappa$ be the cyclic constant, I the given force resultant of the impulse, and $\mu$ the given rotational moment. We have (§ 28) approximately

$$\mathrm{I} = \kappa\pi a^2, \ \mu = \kappa\mathrm{N}\pi r^2 a.$$

Hence

$$a = \sqrt{\frac{\mathrm{I}}{\kappa\pi}}, \ r = \sqrt{\frac{\mu}{\mathrm{N}\kappa^{\frac{1}{2}}\pi^{\frac{1}{2}}\mathrm{I}^{\frac{1}{2}}}},$$

$$\tan\theta = \sqrt{\frac{\mathrm{I}^{\frac{3}{2}}}{\mathrm{N}\mu\kappa^{\frac{1}{2}}\pi^{\frac{1}{2}}}}.$$

**11.** Suppose, now, instead of a single thread wound spirally round a toroidal core, we have two separate threads forming as it were a "two-threaded screw," and let each thread make a whole

ring, or any solid with a single hole through it, may be called a toroid; but to deserve this appellation it had better be not very unlike a tore.

The endless closed axis of a toroid is a line through its substance passing somewhat approximately through the centres of gravity of all its cross sections. An apertural circumference of a toroid is any closed line in its surface once round its aperture. An apertural section of a toroid is any section by a plane or curved surface which would cut the toroid into two separate toroids. It must cut the surface of the toroid in just two simple closed curves, one of them completely surrounding the other on the sectional surface: of course, it is the space between these curves which is the actual section of the toroidal substance, and the area of the inner one of the two is a section of the aperture.

A section by any surface cutting every apertural circumference, each once and only once, is called a cross section of the toroid. It consists essentially of a simple closed curve.

number of turns round the toroidal core. The two threads, each endless, will be two helically tortuous rings linked together, and will constitute the core of what will now be a double vortex ring. The formulæ just now obtained for a single thread would be applicable to each thread, if $\kappa$ denoted the cyclic constant for the circuit round the two threads, or twice the cyclic constant for either, and N the number of turns of either alone round the toroidal core. But it is more convenient to take N for the number of turns of both threads (so that the number of turns of one thread alone is $\frac{1}{2}$N), and $\kappa$ the cyclic constant for either thread alone, and thus for very high steady modes of the double vortex ring

$$I = 2\kappa\pi a^2, \quad \mu = \kappa N \pi r^2 a,$$

$$\tan\,\theta = \sqrt{\frac{(\frac{1}{2}I)^{\frac{3}{2}}}{N\mu\kappa^{\frac{1}{2}}\pi^{\frac{1}{2}}}}.$$

Lower and lower steady modes will correspond to smaller and smaller values of N, but in this case, as in the case of the single vortex core, the form will be a curve of some ultratranscendent character, except for very great values of N, or for values of $\theta$ infinitely nearly equal to a right angle (this latter limitation leading to the case of infinitely small transverse vibrations).

12. The gravest steady mode of the double vortex ring corresponds to N = 2. This with the single vortex core gives the case of the twisted ellipse (§ 7). With the double core it gives a system which is most easily understood by taking two plane circular rings of stiff metal linked together. First, place them as nearly coincident as their being linked together permits (fig. 5). Then separate them a little, and incline their planes a little, as shown in the diagram. Then bend each into an unknown shape determined by the strict solution of the transcendental problem of analysis to which the hydro-kinetic investigation leads for this case.

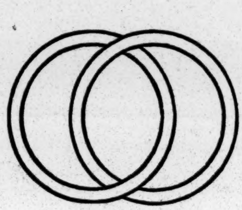

Fig. 5.

13. Go back now to the supposition of § 11, and alter it to this:—

Let each thread make one turn and a half, or any odd number of half turns, round the toroidal core: thus each thread will have an end coincident with an end of the other. Let these coincident ends be united. Thus there will be but one endless thread making an odd number N of turns round the toroidal core. The cases of $N = 3$ and $N = 9$ are represented in the annexed diagrams (fig. 9).*

Imagine now a three-threaded toroidal helix, and let N denote the whole number of turns round the toroidal core, we have

$$I = 3\kappa\pi a^2, \ \mu = \kappa N \pi r^2 a,$$

$$\tan\theta = \sqrt{\frac{(\frac{1}{3}I)^{\frac{3}{2}}}{N\mu\kappa^{\frac{1}{2}}\pi^{\frac{1}{2}}}}.$$

Suppose now N to be divisible by 3: then the three threads form three separate endless rings linked together. The case of $N = 3$ is illustrated by the annexed diagram (fig. 6), which is repeated from the diagram of V. M. § 58. If N be not divisible by 3, the three threads run together into one, as illustrated for the case of $N = 14$ in the annexed diagram (fig. 7).

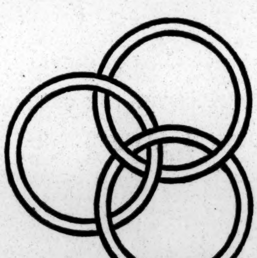

Fig. 6.

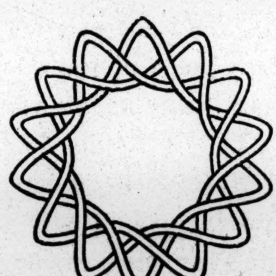

Fig. 7.

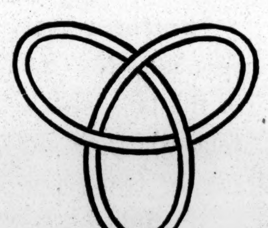

Fig. 8. "Trefoil Knot."

14. The irrotational motion of the liquid round the rotational cores in all these cases is such that the fluid velocity at any point is equal to, and in the same direction as, the resultant magnetic force at the corresponding point in the neighbourhood of a closed gal-

* The first of these was given in § 58 of my paper on vortex motion. It has since become known far and wide by being seen on the back of the "Unseen Universe."

vanic circuit, or galvanic circuits, of the same shape as the core or cores. The setting forth of this analogy to people familiar, as modern naturalists are, with the distribution of magnetic force in the neighbourhood of an electric circuit, does much to promote a clear understanding of the still somewhat strange fluid motions with which we are at present occupied.

15. To understand the motion of the liquid in the rotational core itself, take a piece of Indian-rubber gas-pipe stiffened internally with wire in the usual manner, and with it construct any of the forms with which we have been occupied, for instance the symmetrical trefoil knot (fig. 8, § 13), uniting the two ends of the tube carefully by tying them firmly by an inch or two of straight cylindrical plug, then turn the tube round and round, round its sinuous axis. The rotational motion of the fluid vortex core is thus represented. But it must be remembered, that the outer form of the core has a motion perpendicular to the plane of the diagram, and a rotation round an axis through the centre of the diagram, and perpendicular to the plane in each of the cases represented by the preceding diagrams. The whole motion of the fluid, rotational and irrotational, is so related in its different parts to one another, and to the translational and rotational motion of the shape of the core, as to be everywhere slipless.

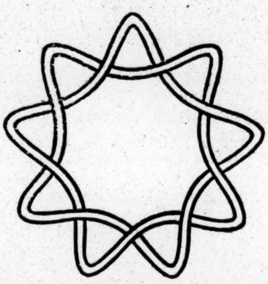

Fig. 9. "Nine-leaved Knot."

16. Look to the preceding diagrams, and, thinking of what they represent, it is easy to see that there must be a determinate particular shape for each of them which will give steady motion, and I think we may confidently judge that the motion is stable in each, provided only the core is sufficiently thin. It is more easy to judge of the cases in which there are multiple sinuosities by a synthetic view of them (§ 3) than by consideration of the maximum-minimum problem of § 8.

17. It seems probable that the two- or three- or multiple-threaded toroidal helix motions cannot be stable, or even steady, unless I, $\mu$, and N are such as to make the shortest distances between different positions of the core or cores considerable in

comparison with the core's diameter. Consider, for example, the simplest case (§ 12, fig. 5) of two simple rings linked together.

18 Go back now to the simple circular Helmholtz ring. It is clear that there must be a shape of absolute maximum energy for given vorticity and given impulse, if we introduce the restriction that the figure is to be a figure of revolution, that is to say, symmetrical round a straight axis. If the given vorticity be given in this determinate shape the motion will be steady, and there is no other figure of revolution for which it would be steady (it being understood that the impulse has a single force resultant without couple). If the given impulse, divided by the cyclic constant, be very great in comparison with the two-thirds power of the volume of liquid in which the vorticity is given, the figure of steadiness is an exceedingly thin circular ring of large aperture and of approximately circular cross section. This is the case to which chiefly attention is directed by Helmholtz. If, on the other hand, the impulse divided by the cyclic constant be very small compared with the two-thirds power of the volume, the figure becomes like a long oval, bored through along its axis of revolution and with the ends of the bore rounded off (or trumpeted) symmetrically, so as to give a figure something like the handle of a child's skipping-rope, but symmetrical on the two sides of the plane through its middle perpendicular to its length. It is certain that, however small the impulse, with given vorticity the figure of steadiness thus indicated is possible, however long in the direction of the axis and small in diameter perpendicular to the axis and in aperture it may be. I cannot, however, say at present that it is certain that this possible steady motion is stable, for there are figures not of revolution, deviating infinitely little from it, in which, with the same vorticity, there is the same impulse and the same energy, and consideration of the general character of the motion is not reassuring on the point of stability when rigorous demonstration is wanting.

19. Hitherto I have not indeed succeeded in rigorously demonstrating the stability of the Helmholtz ring in any case. With given vorticity, imagine the ring to be thicker in one place than in another. Imagine the given vorticity, instead of being distributed in a symmetrical circular ring, to be distributed in a ring still,

with a circular axis, but thinner in one part than in the rest. It is clear that with the same vorticity, and the same impulse, the energy with such a distribution is greater that when the ring is symmetrical. But, now let the figure of the cross section of the ring, instead of being approximately circular, be made considerably oval. This will diminish the energy with the same vorticity and the same impulse. Thus, from the figure of steadiness we may pass continuously to others with same vorticity, same impulse, and same energy. Thus, we see that the figure of steadiness is, as stated above, a figure of maximum-minimnm, and not of absolute maximum, nor of absolute minimum energy. Hence, from the maximum-minimum problem we cannot derive proof of stability.

20. The known phenomena of steam rings and smoke rings show us enough of, as it were, the natural history of the subject to convince us beforehand that the steady configuration, with ordinary proportions of diameters of core to diameter of aperture, is stable, and considerations connected with what is rigorously demonstrable in repect to stability of vortex columns (to be given in a later communication to the Royal Society) may lead to a rigorous demonstration of stability for a simple Helmholtz ring if of thin enough core in proportion to diameter of aperture. But at present neither natural history nor mathematics gives us perfect assurance of stability when the cross section is considerable in proportion to the area of aperture.

21. I conclude with a brief statement of general propositions, definitions, and principles used in the preceding abstract, of which some appeared in my series of papers on vortex motion communicated to the Royal Society of Edinburgh in 1867–68 and 69, and published in the Transactions for 1869. The rest will form part of the subject of a continuation of that paper, which I hope to communicate to the Royal Society before the end of the present session.

Any portion of a liquid having vortex motion is called *vortex core*, or, for brevity, simply "core." Any finite portion of liquid which is all vortex core, and has contiguous with it over its whole boundary irrotationally moving liquid, is called *a vortex.* A vortex thus defined is essentially a ring of matter. That it must

be so was first discovered and published by Helmholtz. Sometimes the word *vortex* is extended to include irrotationally moving liquid circulating round or moving in the neighbourhood of vortex core; but as different portions of liquid may successively come into the neighbourhood of the core, and pass away again, while the core always remains essentially of the same substance, it is more proper to limit the substantive term *a vortex* as in the definition I have given.

22. *Definition I.*—The circulation of a vortex is the circulation [V.M. § 60 (*a*)] in any endless circuit once round its core. Whatever varied configurations a vortex may take, whether on account of its own unsteadiness (§ 1 above), or on account of disturbances by other vortices, or by solids immersed in the liquid, or by the solid boundary of the liquid (if the liquid is not infinite), its "circulation" remains unchanged [V. M. § 59, Prop. (1)]. The circulation of a vortex is sometimes called its *cyclic constant.*

*Definition II.*—An axial line through a fluid moving rotationally, is a line (straight or curved) whose direction at every point coincides with the axis of molecular rotation through that point [V. M. § 59 (2)].

Every axial line in a vortex is essentially a closed curve, being of course wholly without a vortex.

23. *Definition III.*—A closed section of a vortex is any section of its core cutting normally the axial line through every point of it. Divide any closed section of a vortex into smaller areas; the axial lines through the borders of these areas form what are called vortex tubes. I shall call (after Helmholtz) a vortex filament any portion of a vortex bounded by a vortex tube (not necessarily infinitesimal). Of course, a complete vortex may be called therefore a vortex filament; but it is generally convenient to apply this term only to a part of a vortex as just now defined. The boundary of a complete vortex satisfies the definition of a vortex tube.

A complete vortex tube is essentially endless. In a vortex filament infinitely small in all diameters of cross sections "rota-

tion" varies [V. M. § 60 (e)] from point to point of the length of the filament, and from time to time inversely as the area of the cross section. The product of the area of the cross section into the rotation is equal to the circulation or cyclic constant of the filament.

24. Vorticity will be used to designate in a general way the distribution of molecular rotation in the matter of a vortex. Thus, if we imagine a vortex divided into a number of infinitely thin vortex filaments, the vorticity will be completely given when the volume of each filament and its circulation, or cyclic constant, are given; but the shapes and positions of the filaments must also be given in order that, not only the vorticity, but its distribution, can be regarded as given.

25. The vortex density at any point of a vortex is the circulation of an infinitesimal filament through this point divided by the volume of the complete filament. The vortex density remains always unchanged for the same portion of fluid. By definition it is the same all along any one vortex filament.

26. Divide a vortex into infinitesimal filaments inversely as their densities so that their circulations are equal; and let the circulation of each be $\frac{1}{n}$ of unity. Take the projection of all the filaments on one plane. $\frac{1}{n}$ of the sum of the areas of these projections is (V. M. §§ 6, 62) equal to the component impulse of the vortex perpendicular to that plane. Take the projections of the filaments on three planes at right angles to one another, and find the centre of gravity of the areas of these three sets of projections. Find, according to Poinsot's method, the resultant axis, force, and couple of the three forces equal respectively to $\frac{1}{n}$ of the sums of the areas, and acting in lines through the three centres of gravity perpendicular to the three planes. This will be the resultant axis; the force resultant of the impulse, and the couple resultant of the vortex.

The last of these, that is to say, the couple is also called the rotational moment of the vortex (V. M. § 6).

27. *Definition IV.*—The moment of a plane area round any axis is the product of the area multiplied into the distance from that axis of the perpendicular to its plane through its centre of gravity.

*Definition V.*—The area of the projection of a closed curve on the plane for which the area of projection is a maximum will be called the area of the curve, or simply the area of the curve. The area of the projection on any plane perpendicular to the plane of the resultant area is of course zero.

*Definition VI.*—The resultant axis of a closed curve is a line through the centre of gravity, and perpendicular to the plane of its resultant area. The resultant areal moment of a closed curve is the moment round the resultant axis of the areas of its projections on two planes at right angles to one another, and parallel to this axis. It is understood, of course, that the areas of the projections on these two planes are not evanescent generally, except for the case of a plane curve, and that their zero values are generally the sums of equal positive and negative portions. Thus their moments are not in general zero.

Thus, according to these definitions, the resultant impulse of a vortex filament of infinitely small cross section and of unit circulation is equal to the resultant area of its curve. The resultant axis of a vortex is the same as the resultant axis of the curve, and the rotational moment is equal to the resultant areal moment of the curve.

28. Consider for a moment a vortex filament in an infinite liquid with no disturbing influence of other vortices, or of solids, immersed in the liquid. We now see from the constancy of the impulse (proved generally in V. M. § 19) that the resultant area, and the resultant areal moment of the curve formed by the filament, remain constant, however its curve may become contorted; and its resultant axis remains the same line in space. Hence, whatever motions and contortions the vortex filament may experience, if it has any motion of translation through space this motion must be on the average along the resultant axis.

29. Consider now the actual vortex made up of an infinite number of infinitely small vortex filaments. If these be of volumes inversely proportional to their vortex densities (§ 25), so that their circulations are equal, we now see from the constancy of the impulse that the sum of the resultant areas of all the vortex filaments remains constant; and so does the sum of their rotational moments: and the resultant areal axis of them all regarded as one system is a fixed line in space. Hence, as in the case of a vortex filament, the translation, if any, through space is on the average along its resultant axis. All this, of course, is on the supposition that there is no other vortex, and no solid immersed in the liquid, and no bounding surface of the liquid near enough to produce any sensible influence on the given vortex.

## 2. Experiments illustrating Rigidity produced by Centrifugal Force. By John Aitken, Esq.

If an endless chain is hung over a pulley and the pulley driven at a great velocity, it is well known that the motion so communicated to the chain has almost no tendency to change the form of the curve in which the chain hangs, and that the principal effect of the motion is to confer on the chain a quasi-rigidity which enables it to resist any force tending to alter its curvature.

This is only true in a general sense, and possibly may be true of some ideal form of chain; but in all chains we can experiment on there are forces in action in the moving chain which tend to cause the chain to depart from the form which it has while at rest.

I shall refer to these disturbing forces later on. As the disturbing forces in most chains are very small, we shall neglect them, and for the present suppose the centrifugal force just balances the tension at all points. The following experiments were made to illustrate the balance of these forces, to show that into whatever curves we may bend the chain when in motion, the centrifugal force has no tendency to alter these curves: that all forms are forms of stability, as far as the centrifugal force is concerned.

The first experiments were to show the effect of destroying the balance between the tension and the centrifugal force. In these experiments the links on the descending side of the loop were

allowed to fall on a platform, so that part of the chain lay loose on the platform, thus destroyed the tension produced by the centrifugal force at the lower part of the chain. The chain was made to take the same velocity as the driving pulley, by being pressed into contact with it by means of an elastic wheel.

I. When the chain was pressed at the point where it leaves the pulley there was no alteration in the path of the chain, because the chain after it leaves the pulley is moving in a straight line, and as there is no deviating force, there is no centrifugal force, and therefore, removing the tension in the chain has no effect on the direction of the motion of the links.

II. When the chain was pressed at a point a little higher up the pulley, then the centrifugal force of the curved part of the chain resting on the pulley at the descending side, being unbalanced by the tension, rises from the pulley, and is shot in a direction away to one side of the pulley. Of course the curved part of the chain on the other side of the pulley has also a tendency to rise, but is kept in its place by the tension produced by putting the chain in motion after being stopped by the platform.

III. When the chain is pressed on the ascending side of the pulley, then the chain rises up off the pulley and forms itself into a somewhat irregular curve resting on the platform, and touching the pulley at only one point. When the velocity is sufficient to raise it to a certain height, the conditions become altered. The chain in rising takes up all the slack chain lying on the platform, and a tension is produced in the chain by the centrifugal force, and unless we keep increasing the speed of the chain, it can no longer keep in its elevated position, because the centrifugal force is now balanced by the tension, and as the force of gravitation is now unbalanced, it gradually flattens the curve till the chain again comes to rest on the top of the pulley and spreads itself out in an irregular curve on the platform.

IV. At the beginning of the previous experiment the centrifugal force being unbalanced by the tension, it overcomes the force of gravitation and causes the chain to rise into the air. After all the slack chain has been taken up, and a tension is produced in the chain by the centrifugal force, then the centrifugal force is balanced by the tension and is no longer capable of opposing gravitation, and

the chain begins to fall; but at this point its fall may be stopped, or the chain may be made to rise again by destroying the tension at the lower part of the chain. If we cause the chain, instead of meeting the platform at an acute angle, to strike it as near as possible at right angles, then the motion of the chain where it strikes the platform is partly destroyed, and the chain again rises and may be kept balanced for a long time resting on the platform, and only touching the driving wheel at one point. The reason for this being, that if we partially stop the motion of the links by causing them to strike the platform, or if we alter the direction of their motion by causing them to strike the platform, then there will be less tension in the lower part of the chain than in the upper, as the tension in the lower part will be only that due to partially changing the direction of the motion of the links. The centrifugal force of the upper part of the chain will be therefore unbalanced, and will cause the chain to rise and keep its elevated position against the force of gravitation. If a quick upward motion is given to the platform, the chain may be thrown up in the air, and again dropped on the platform like a solid body.

The next experiments are to show that centrifugal force may produce sufficient rigidity to cause a chain to run along a platform like a wheel. A short endless chain was put over a pulley which was driven at a great velocity; the chain was then dropped on the platform, along which it ran for some distance. It is not necessary that the chains form circular loops to do this. The loop may be tall and narrow, and will, while running along, keep the longer axis of the curve in its original upright position. Nor need the chain be heavy. A watch-guard was hung over a pulley about eight inches diameter; it then formed a loop about eight inches broad by about two feet high. When thrown off the pulley it glided along the platform for some distance. The chains were also dropped on an inclined polished surface, on which they remained standing in rapid motion for some time.

All these experiments only illustrate the balance of the centrifugal force, and the tension when the motion is all in one plane. The next experiment is to illustrate this balance when the motion takes place in different planes. This is easily illustrated by means of a circular disc of paper, or any other flexible material. If we

bend the disc while it is rotating, we find that the bent part does not rotate with the disc, and that the disc only slowly regains its original flat form. If we load the outside of the disc with a row of flattened pellets of shot, we increase the resistance or rigidity of the disc while in motion, and if the weight is such that it just balances the elasticity of the paper, then the bend will remain in the same place for a very long time while the disc is rotating rapidly. The disc may even be bent till the circumference touches the centre, and while the bend keeps its place the chain of shot is passing through many planes, and the tension at the different points just balances the centrifugal force.

Before proceeding to experiment with the horizontal chain, I must refer to the disturbing forces in action causing the chain to change its form while in motion. When looking at these endless chains in motion, the most marked effect of this motion is to cause a curious reverse curve just after the chain has turned at the lowest point of its path and has begun to ascend. This reverse curve was supposed to be produced by friction from the great tension produced by the centrifugal force; but that it is not really so, is easily proved by taking two precisely similar chains and oiling one and passing the other through a flame to remove all grease. The only difference between the two chains now is that the friction in the one is greater than in the other. If we hang these two chains over two pulleys of the same diameter on the same shaft so as to drive both chains at the same velocity, we find that the oiled chain has the reverse curve well marked, while the friction in the other chain causes the loop to open out and take up a curve approaching a circle and shows no reverse curve, and when both chains are compelled to have the same curvature at bottom, the reverse curve is much the least where the friction is greatest. The reverse curve seems to be due to the change of motion which takes place in the links when moving in a path of varying curvature. For instance, when the links are descending along the flat part of the curve, their motion is almost simply one of translation, whereas when passing round the curves they have a motion of rotation as well as a motion of translation, the result of which is, that the links resist this rotation at the entrance of the curve, and thus flatten out the curve on that side, and after the rotation has been communicated to them, they tend to

keep this rotation, and thus continue the curve at the lower end of the chain much farther round than if the chain was not in motion. And, for very evident reasons, the quickest part of the curve is not at the bottom, but a short distance up the ascending side; and farther, the rotation of the link at the bottom is quicker than that corresponding to the curvature. These points may all be illustrated by a chain in which the links are short and the chain as thick as possible, so that the moment of inertia of the links round an axis perpendicular to the plane of the motion of the links is as great as possible. Such a chain when properly made gives a series of large and well-marked waves all the way up the one side of the loop and down the other. The length of the links also tends to change the form of the curve. If we have two chains of the same material and same size every way, except in the length of the link, then the larger the link the more the chain tends to open out the curves and take the circular form, and the smaller the links the nearer it approaches the form it has while at rest, and the more marked the reverse curve becomes.

An elastic band in rapid motion will also tend to take up a circular form, because the strain at the quick part of the curves will tend to open them out, in the same manner as when the band was at rest. An elastic chain while in motion does not show the reverse curve like a chain, probably because the strain on the material prevents it doing so.

In the previous experiments gravitation acted on the chains, so that whatever form we might impress on them, gravitation constantly tended to change that form and bring it back nearly to the form it would have if gravitation alone acted on it. An attempt was therefore made to get quit of the disturbing effect of gravitation. Different ways were tried of effecting this, but none of them were thoroughly successful. The next experiment shows the most successful method tried, namely, suspension. The chain is hung by means of a number of fine cords to a circular disc, capable of rotating about a vertical axis placed as far above the chain as possible. The chain is driven by means of a rapidly revolving horizontal pulley running on a vertical axis, and to give sufficient friction the chain is pressed to the pulley by means of an elastic wheel. The centre of suspension is so arranged that it can be

brought over the driving pulley, or removed to some distance from it, so as to be able to bring the centre of suspension over the centre of gravity of the chain, whatever shape the chain may be caused to take. The chain so suspended, when in rapid motion, retains for a considerable time whatever form we please to give it. It may be moulded into a most complicated series of curves, and though it resists any effort made to alter these curves, it has itself but little tendency to do so. If we observe the chain closely, we will however find that the disturbing forces, which I have already referred to, are acting on the chain, tending to change its curvature. For instance, if we keep the point of suspension over the centre of gravity of the chain, we will find after some time that the chain will take up a circular form. This is caused by the friction in the chain, and other causes. Again, the effect of the varying rate of rotation of the link on its own axis is also well marked, but is quite different from what we get when the chain is hung over the driving pulley. When the chain is hung over the pulley, there is a tension due to the weight of the chain. This tension gives rise to the wave form which certain chains take up when in motion. The tension due to the centrifugal force has no such effect. When, therefore, the chain is suspended and gravitation removed, there is no tension preventing the chain from continuing to curve always in the same direction; and if we use a chain specially prepared to show this effect, such as the one already referred to, the chain goes on bending further and further round till it comes against the part of the chain coming in the opposite direction and stops the motion, even though the chain at that point is also bending out of the way on account of the resistance offered by the links to rotation on their axis.

*Monday, 3d January* 1876.

Sir WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. On the Electrical Conductivity of Stretched Silver Wires. By J. G. MacGregor, M.A., B.Sc. Communicated by Professor Tait.

The apparatus which I used in a few experiments on silver wires was as follows:—To a beam, supported in stonework, a plate of copper was fastened, upon which a smaller plate could be tightly screwed. Between the two plates a very thick copper wire was secured, vertically. Its lower end was provided with a small plate of copper, fastened by screws. This plate served to make fast one end of the silver wire under investigation. The other end was joined in the same way to a second thick copper wire; this was provided with a horizontal round brass plate, through the centre of which it passed, and which acted as weight-carrier. A length of about 8 mm. at the end of the part of the copper wire which projected below the weight-carrier was amalgamated, and, while hanging quite free, dipped into a glass cup containing mercury, which, by means of a long screw, could be elevated or depressed by any desired amount. When measurements of resistance were made it was always placed in such a position that the amalgamated part of the copper wire was just beneath the surface of the mercury. The glass cup served also to support the weight-carrier during the adjustment of the weights, that the silver wires might be subjected to no jerks. After putting on weights the cup was lowered very slowly and steadily until the weights hung free. A copper wire (4·7 mm. thick and 30 cm. long), dipping in the mercury, joined up the silver wire as one of the arms of a Wheatstone's bridge. At the upper end of the copper wire, which was fastened to the beam, two other copper wires were fastened by binding screws. One of them went to the galvanometer; the other was the standard wire, with whose resistance that of the silver wires was

compared. For all the observations on a single wire, it had, in all cases, as nearly as possible the same temperature. That it might not be affected by warm or cold currents of air it was defended by a coating of gutta percha, and made to pass through a tube of water whose temperature could readily be noted. By dipping into a mercury pool it was joined up as a second arm of the Wheatstone's bridge. A length of about 5 mm. of the end which dipped in the mercury was well amalgamated. Above that the wire was varnished by a non-conductor, so that contact began always at the same point of the wire. The other two arms of the bridge consisted of the segments of a German-silver wire,—Kirchhoff's form of the Wheatstone bridge being used exactly as described by Wiedemann in his "Galvanismus."* The galvanometer used was Wiedemann's mirror galvanometer,† the deflections of the mirror being observed by means of a telescope. The current employed was that of a Bunsen's cell of great internal resistance. The length of the wire was determined by a very delicate cathetometer, which could measure accurately to ·02 mm. The lower end of the copper wire which was fastened to the beam, was smooth and flat, and cut at right angles to its vertical axis. The edge of the small plate was correspondingly cut, so that the exact point at which the silver wire was seized and compressed by the copper plate could be seen through the telescope of the cathetometer. The clamp which seized the lower end of the silver wire was arranged in the same way. The wires, of whose resistance measurements were made, were of pure silver, and were carefully drawn by M. E. Stöhrer, philosophical instrument maker of Leipzig. They were always raised to a red heat before being subjected to tension, care being taken that fusion did not occur in any part. In order to determine the effect of tension on the conductivity of the wires, it was necessary to know the relation of their diameter before to their diameter after being stretched. This was estimated by a careful measurement of lengths and specific gravities. For the latter purpose a chemical balance was employed, which could weigh accurately to ·0001 grm. As the wires had to be rolled up to prevent their touching the sides of the vessel containing the distilled water in which they were weighed, the measured specific gravity was pro-

* "Galvanismus," vol. i. pp. 251–255, 2d German ed., 1872.

† "Galvanismus," vol. ii. pt. 1, pp. 227–230, 2d Ger. ed., 1873.

bably not exactly that of the wire of measured resistance. The error, however, must have been very slight.

The course of procedure was as follows:—The wire was heated red hot in an alcohol flame. After cooling, its specific gravity was determined. It was then fastened by the copper plates to the thick copper wires, and a slight weight was attached just sufficient to straighten the wire, that its length might be accurately ascertained (it was straightened as much as possible by being drawn between the fingers before being fastened in the apparatus). When straight enough for the determination of its length, its resistance was also measured by the method of double observation, as described in the "Galvanismus" (see above).* After the determination of resistance, weights were carefully piled upon the carrier, which during the operation was held fast from below. They were then allowed to stretch the wire gradually until it hung quite free, and its elongation had ceased. Then the length, the resistance, and finally the length a second time, were determined, the second measurement of length being made in order to be certain that there had been no further elongation. The weights were now carefully taken off, the carrier being again supported from below, and the specific gravity of the wire was measured as before, the compressed ends, however, having been cut off. A large number of experiments were rendered useless by the fact that too great weights were attached. Either the wires broke, or they were found on inspection to have too variable a diameter to be regarded as uniform. In the result given below such small weights were used that almost no difference of diameter throughout the whole length of the wire could be noticed by means of a magnifying instrument. Thus the wire could be treated as uniform, and the specific gravity method assumed to give its diameter.

The results of the examination of three wires are given in the following table:—

* Instead of the formula given in "Galvanismus," the following was used:—

$$s^3+s^2(3d_{12}-d_1-d_2)+s(d_1d_{12}+d_2d_{12}-3d_1d_2)-d_1d_2d_{12}=0.$$

The length of the German-silver wire as found by this formula was 1108·795 mm. As measured by the cathetometer its length was 1108·8 mm.

| | Length (mm.) | Weight (grms.) | d* (mm.) | Resistance (standard copper wire =unity). | Length (length before stretching=unity). | Resistance (resistance before stretching =unity). |
|---|---|---|---|---|---|---|
| Wire I. | 777·62 | 2246 | 198·8 | 1·4369 | 1 | 1 |
| | 809·56 | 5075 | 239·8 | 1·5519 | 1·0411 | 1·0800 |
| Wire II. | 830·82 | 1246 | 234·9 | 1·5376 | 1 | 1 |
| | 890·04 | 6746 | 311·2 | 1·7803 | 1·0713 | 1·1579 |
| Wire III. | 660·86 | 1246 | 114·7 | 1·2308 | 1 | 1 |
| | 729·14 | 7911 | 216·9 | 1·4864 | 1·1033 | 1·2105 |

These results agree, as might be expected, with those which Mousson † has published on steel, iron, and copper wires, in the fact that the resistance increases very much faster than the length. This must be the case unless there be a diminution of resistance, due to tension, sufficient to neutralise the increase of resistance due to decrease of the cross section of the wire. It is interesting to ask, then—Does the decrease in the diameter of the wire account for that part of the increase of its resistance which is not due to the increase of its length? The following table answers this question. The column headed "calculated resistance" contains the resistance as it ought to have been if its increase had been due only to change of dimensions:—

| | Specific Gravity | | Observed resistance after stretching. | Calculated resistance. |
|---|---|---|---|---|
| | Before stretching. | After stretching. | | |
| Wire I. | 10·4784 | 10·5330 | 1·080 | 1·092 |
| Wire II. | 10·4967 | 10·5646 | 1·157 | 1·155 |
| Wire III. | 10·5051 | 10·5394 | 1·210 | 1·220 |

The agreement of the figures in the observed and calculated columns is very close, notwithstanding the many sources of error to which the experiments were liable, such as the change in

* See Wiedemann's "Galvanismus," vol. i. p. 255.

† "Galvanismus," vol. i. p. 310; "Neue Schweizerische Zeitschrift," vol. xiv. (1855), p. 33.

specific gravity produced by the rolling up of the wires, their extension by weight between the first determination of specific gravity and the first determination of resistance, the irregularity in their cross section produced by stretching, and the slight contraction of the wires after the removal of the weights and before the second determination of specific gravity,—all of which, however, must have been exceedingly slight. It seems to warrant the statement that if tension has any effect upon silver wires at all the effect is exceedingly small. This differs from Mousson's conclusion as to steel, iron, and copper wires. He found that the increase in their resistance produced by stretching was not fully accounted for by the change of their dimensions.

In the course of the experiments I found that by raising a silver wire, which had been stretched, to a red heat, its resistance was very slightly diminished. A wire of about the dimensions of No. III., which, after having been stretched by 6985 grms. had a resistance of 1·8135, had, after being heated red hot, a resistance of 1·8103. This is again different from what Mousson has found to be true of steel, iron, and copper wires, but it agrees with a determination made by Becquerel on silver wires.*

The following tables contain series of observations made for the purpose of finding the relation between the stretching weight and the total increase in the resistance of the silver wires used. In these determinations, the constant resistance with which the resistances of the stretched wires were compared was that of a silver wire. Both wires were surrounded by a coating of steam. The stretched wire, in order that, by its being kept at a high temperature, greater elongations might be produced by the same weights; the constant wire in order that thermo-electric effects might be eliminated. The steam coating was formed by enclosing the wires in glass tubes, and these tubes in a much larger tube, and conducting steam between them. In other respects the apparatus and mode of procedure were quite the same as before. The observations were made when the appended weights had ceased to produce any appreciable elongation, and with the steam coating half-an-hour was generally found to be a sufficient length of time for the production of the total stretching effect.

* "Ann. de Chimie et de Physique" (3), xvii. 1846, p. 253.

*able I.*

| Weight (grms.) | d mm. | Resistance (Constant Silver Wire = unity). |
|---|---|---|
| 500 | 102·0 | ·8315 |
| 750 | 99·8 | ·8348 |
| 1000 | 99·6 | ·8351 |
| 1250 | 98·8 | ·8363 |
| 1500 | 95·8 | ·8409 |
| 1750 | 91·4 | ·8477 |
| 2000 | 82·0 | ·8623 |
| 2250 | 63·0 | ·8925 |

*Table II.*

| | | |
|---|---|---|
| 1285 | 60·6 | ·8963 |
| 1535 | 52·6 | ·9094 |
| 1785 | 42·6 | ·9260 |
| 2035 | 26·6 | ·9531 |
| 2285 | — ·6 | 1·0011 |
| 2535 | —42·2 | 1·0791 |

*Table III.*

| | | |
|---|---|---|
| 873 | 4·2 | ·9924 |
| 1285 | — 24·6 | 1·0453 |
| 1535 | — 48·2 | 1·0909 |
| 2035 | —121·4 | 1·2459 |

*Table IV.*

| | | |
|---|---|---|
| 1873 | 678·6 | ·2407 |
| 2873 | 676·8 | ·2419 |
| 3214 | 670·6 | ·2463* |
| 4857 | 663·0 | ·2516 |
| 5171 | 659·6 | ·2540 |
| 5921 | 647·2 | ·2629 |
| 6490 | 635·0 | ·2717 |
| 6964 | 623·4 | ·2802 |
| 7650 | 600·8 | ·2971 |
| 8176 | 578·8 | ·3141 |
| 8307 | 567·0 | ·3233 |
| 8842 | 532·0 | ·3515 |

* This measurement is marked in my notes as "inaccurate, owing to an error of observation."

It will be seen that the relation between appended weights and thereby increased resistances is not that of simple proportion. In this respect silver wires appear again to differ from copper wires. Some experiments made by Messrs Meik and Murray * having shown that the changes of resistance of copper wires, when stretched by weights, are directly proportional to the weights.

I am deeply indebted to Professor Wiedemann of Leipzig, in whose laboratory these experiments were performed, for the excellent apparatus which he kindly placed at my disposal, and for the advice and assistance with which he favoured me.

## 2. On the Defoliation of the Coniferæ. By Dr Stark.

## 3. On Diamagnetic Rotation. By George Forbes, Esq., M.A., F.R.A.S.

Faraday's discovery of the magnetic rotatory polarisation of light may be expressed in the following manner:—Let two electro-magnets, in the form of iron tubes, surrounded by helices of wire, be placed end to end, so that in the space between them the lines of force are very intense. Let a rod of dense glass be placed in this space, so that a ray of light may pass through the two tubes and the rod of glass. Let such a ray on entrance be plane-polarised, so that the direction of vibration is in a vertical direction. If the electro-magnet be now magnetised, the emergent ray will be polarised, so that its vibrations are inclined to the vertical at a small angle. The direction in which the line of vibration has been rotated is the same as the direction of the positive current in the helices.

The same effect might be produced without the aid of magnetism if the rod were rotated round the axis of the ray of light with great velocity. The rotation of the plane of polarisation

* Proc. Roy. Soc. Edin., Session 1869–70, p. 3.

would be the angle rotated whilst the light traverses the glass rod. This is on the assumption that the ether within the rod is likewise rotated. In the magnetic experiment it is easy to produce a rotation of 1° in a piece of glass three inches long. Light takes $\frac{1}{4,000,000,000}$ of a second to traverse this distance. Hence, to produce an effect equal to the magnetic effect, the glass rod would require to be rotated 10,000,000 times in a second. We cannot determine with great precision the plane of polarisation of a ray of light, hence we cannot measure any rotation of the plane of polarisation which might be thus produced.

In the same manner, if we suppose the molecules of glass and the accompanying ether to be rotated round the lines of magnetic force, and in the direction of the positive current producing the given magnetic field, then the phenomena observed by Faraday would be explained; and we should be able to determine the number of rotations per second induced in any specimen of glass with a given intensity of magnetic field.

So soon as the electro-magnet is demagnetised, the rotation of the molecules ceases. It seems, then, that there is a friction among the molecules tending to stop the rotation. Hence we should be led to conclude that the energy of the magnet is gradually used up by the presence of the piece of glass.

Assuming, then, that there is a friction among the molecules of glass, it follows that when the electro-magnet is magnetised the rod of glass has a tendency to turn bodily round an axis through its centre of gravity parallel to the lines of force; and if the rod of glass were free it would turn round this axis.

In the winters 1872–3 and 1873–4, I made a number of experiments to put this hypothesis to the test. The general idea of the experiments was this. A rod of glass was suspended by a fine skein of silk fibres between two poles of an electro-magnet, one pole above, the other below. A small mirror was fixed on the rod, and a lamp and scale arrangement was mounted for measuring rotations. Readings were taken when there was no current, and also in the two positions of the commutator.

The result of these experiments seems to be that there is an effect of the kind anticipated. Sometimes, it is true, a deflection was produced by a diamagnetic repulsion, owing to a want of

absolute symmetry in the arrangements. This rotated the glass rod slightly, and produced a little confusion. But beyond this there was an undoubted effect, for the nature of the deflection was found to depend upon the polarity of the two poles, the effect being different according as the upper pole was a north or a south one. I was sometimes able, simply by timing the reversals of the commutator, to get up a very large rotation-swing, and by the same means I was able to stop the rotation swinging.

I delayed the publication of these experiments with the view of establishing more certain results. But much time has elapsed, and I have still been unable to find time for this. Hence, I am unwilling any longer to withhold their publication. I will now give the experiments in detail, and will conclude by collecting the general results.

### *Description of the Apparatus.*

A rod of glass was suspended by a strand of silk fibres attached to one end. This was supported on a stone imbedded in the wall of the laboratory. The rest of the apparatus was on a separate stand. A horse-shoe electro-magnet was placed so that the poles were vertically over each other, and as nearly above and below the axis of the glass rod as possible. Thus the axis of the glass rod lay along the lines of magnetic force. The electro-magnet was connected through a commutator with a battery of Grove cells. Upon the glass rod was attached a small piece of silvered glass, by means of which the light reflected from a paraffin lamp fell upon a scale divided to millimetres. The distance of the scale from the glass rod was about seven decimetres. The apparatus was arranged so that the glass rod was suspended within a glass jar or bottle, to get rid of currents of air.

During the session 1872–73 thick flint glass tubes were employed for suspension, and an electro-magnet weighing about 6 lbs. After that a rod of Faraday's heavy glass was used, and an electro-magnet weighing about 50 lbs.

*Details of Experiments.*

The first experiments were made in 1873, March 28, and they were continued regularly until April 16. The rod of flint-glass was suspended in air, water, and oil. A rotation was nearly always communicated to the rod when contact was made with the electro-magnet. When the magnetism of the electro-magnet was reversed I sometimes got a rotation in the opposite direction, and sometimes a rotation in the same direction, but to a different extent. The apparatus was repeatedly dismounted and set up again with all the parts changed. The effects were generally unaltered. In noting the experiments, I used the terms "no contact," "right contact," and "left contact," as being least liable to lead to error. "No contact" means that the circuit was broken. "Right contact" means that the lower pole of the electro-magnet would, if free, point to the north. "Left contact" means that the lower pole of the magnet would, if free, point to the south. When the readings of the scale are increasing the glass is rotating in the direction of the hands of a watch with the face upwards. The glass was sometimes put into water or oil, and in this case also rotation was generally observed.

The rotation oscillations in air were so great as to necessitate reading the scale at the end of each half oscillation, and taking a mean. In the following table each reading is a mean of many observations:—

*March* 29.—*Flint-Glass in Air.*

| | | Means. |
|---|---|---|
| No contact, . . | 245 scale reading. | |
| Right „ . . | 155 | L = 146. |
| Left „ . . | 146 | R = 155. |
| No „ . . | 235 | N = 240. |

*Flint-Glass in Water.*

| | | |
|---|---|---|
| No contact, . . | 415 | |
| Right „ . . | 268 | L = 248. |
| Left „ . . | 248 | R = 268. |
| No „ . . | 410 | N = 412. |

*Flint-Glass in Oil.*

Right and left contact each continued to diminish the scale reading during twenty-two minutes. The change from left to right contact diminished the readings with a jump. "No contact" again reversed the rotation.

*Flint-Glass in Water.*

No decided effect.

*Flint-Glass in Air.*

| | | | |
|---|---|---|---|
| No contact, . . | 370 | scale reading. | Means. |
| Right „ . . | 523 | | N = 370. |
| Left „ . . | 478 | | L = 478. |
| No „ . . | 370 | | R = 523. |

The poles of the electro-magnet were now put so far to the east of the glass as possible, and again as far to the west. The same kind of results were obtained.

Oil was again used, and no certain effect was observed. *March* 31.—The apparatus, as left last day, with oil in the jar, showed no effect. No effect was shown with air. But on trying a new rod of flint-glass the old effects were observed. The oil seemed to have destroyed the effect.

*Flint-Glass in Air.*

Right contact, steady rise from 400 to 543 in about 4 minutes.

| | | |
|---|---|---|
| No contact, . . . | 558 | |
| Left „ . . . | 567 | |
| Right „ . . . | 546 | R = 538. |
| Left „ . . . | 556 | N = 542. |
| No „ . . . | 540 | L = 552. |
| Right „ . . . | 531 | |
| No „ . . . | 529 | |
| Left „ . . . | 533 | |

Half-an-hour interval.

| | | |
|---|---|---|
| No contact, . . . | 522 | |
| Right „ . . . | 512 | R = 509. |
| Left „ . . . | 523 | N = 515. |
| Right „ . . . | 497 | L = 521. |
| Left „ . . . | 519 | |
| No „ . . . | 508 | |

By keeping right contact for decreasing rotation-swing, and left for increasing, the range of swing increased after 6 swings to 123 divisions. On reversing this action for 6 swings the range was reduced to 12 divisions.

*Note.*—These are the most striking experiments. It will be noted that here the "right" gives lower readings than the "left" contact. But I cannot be sure that the commutator arrangements had not been changed. Here all disturbing effects are done away with, and the right and left contacts produce deflections on opposite sides of the "No contact" reading.

*Flint-Glass in Water.*

| | | |
|---|---|---|
| No contact, . . . | 541 scale reading. | Means. |
| Right " . . . | 531 | R = 531. |
| Left " . . . | 551 | N = 541. |
| No " . . . | 540 | L = 551. |

A new rod was now prepared, which also showed a very decided difference between right and left contact, right being lowest.

Numerous experiments under various conditions were now tried, but no new effects were discovered. It is desirable, however, to record the following, which were made to be certain as to the direction of rotation under different conditions of polarity:—

| | |
|---|---|
| No contact, . . . . . . . . . . | 850 |
| Upper pole would point to north if free, . . | 840 |
| No contact, . . . . . . . . . . | 848 |
| Lower pole, . . . . . . . . . . | 854 |
| Upper pole, . . . . . . . . . . | 835 |
| Lower pole, . . . . . . . . . . | 850 |
| No contact, . . . . . . . . . . | 845 |

The general result of the experiments of the winter is, that *when a pole tending to point to the north is above the glass rod, and a pole tending to point to the south is below, the rod turns in the opposite direction to the hands of a watch with the face upwards.*

On placing the magnet with the poles in a horizontal plane, rotations were sometimes observed. This is in direct contradiction to the theory that led to these experiments. I can offer no explanation, but merely record the fact.

In the session 1873–74 similar experiments were made with a

piece of Faraday's heavy glass. But in this case the glass was inserted in a hole in a cork, to which also the mirror was attached. On examining the cork it was found to be magnetic. This accounted for all the phenomena observed.

But in the previous session this would not account for the phenomena. There was no cork employed; and experiments made to test this by altering the position of the magnet showed that there was no magnetism in the arrangement.

It was the doubt that hung over the last winter's experiments that made me wish to delay publishing any results until I should have finally settled the matter. I have been unable to do so hitherto, and offer the original experiments in the meantime.

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*Note on the preceding paper.*

[The first statement is that a rotation of the plane of polarised light might be produced by rotating a transparent body about the ray as an axis.

It is improbable that no such effect would be produced, but that the question is by no means a simple one may be seen by looking at Sir W. Thomson's paper on this subject (Proc. R. S. Lond., 1856).

I have also tried a great many hypotheses besides those which I have published, and have been astonished at the way in which conditions likely to produce rotation are exactly neutralised by others not seen at first.

There can be no doubt, however, that a rotation of some kind is going on in a diamagnetic medium under magnetic force, and this may be of the molecules of the glass of the ether or what not, and this probably goes on in all media whether transparent or not.

This rotation, as Prof. George Forbes says, stops as soon as the magnetic force is removed. He supposes that it is stopped by friction, and therefore, that energy is being dissipated at all times as long as magnetic force acts on a medium.

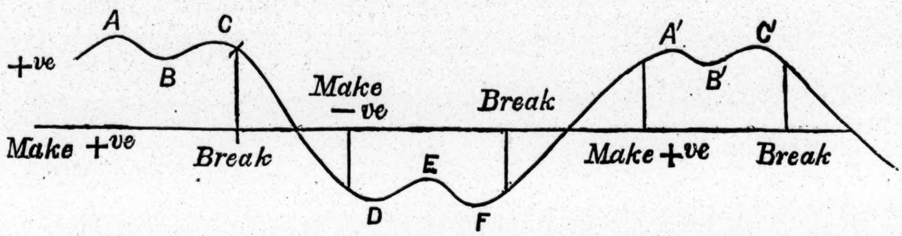
But we know that a magnet will retain its magnetism for a long time, and it has never been shown that a magnet must necessarily lose its magnetism. Hence we must admit that the molecular rotation is not accompanied with friction, but that it is set up by

electro-motive force, and exerts electro-motive force when it is stopped, like a rotating body having inertia.

($\alpha$) If the friction supposed by Prof. Forbes exists, it would act as an accelerating force on the glass, so that if free it would rotate faster and faster up to a certain great velocity, and if suspended by a fibre, it would rotate till the moment of friction was balanced by the moment of tortion of the fibre.

($\beta$) If there is no friction the only effects possible would be those due, not to the maintenance, but to the starting and stopping of the molecular rotation.

To investigate ($\alpha$) experimentally we must observe the elongations of the oscillation as follows:—




Make $+^{ve}$ observe three turning points A B C, break for nearly half a complete vibration. Make $-^{ve}$ observe three turning points D E F, break again, and make $+^{ve}$, and so on. Then the result is obtained by taking

$$\frac{1}{8n}\Sigma\{A+2B+C-(D+2E+F)\}$$

when $n$ represents the number of repetitions of the series of six observations.

To investigate ($\beta$) experimentally we must make and break when the mirror is passing the point of equilibrium.

In Prof. Forbes's experiments there is a disturbing effect due to the ordinary diamagnetic action of the electro-magnet on the tube, which, if the tube is not perfectly symmetrised about the axis of the fibre, will tend to produce rotation. This force, however, is the same whether the current be + or −, provided the position of the tube is the same. Hence, if the + and − currents are exactly equal, it may be possible to distinguish this effect from the effect sought by Prof. Forbes.

J. Clerk Maxwell.]

## 4. On the Linear Differential Equation of the Second Order. By Professor Tait.

(*Abstract.*)

This paper contains the substance of investigations made for the most part many years ago, but recalled to me during last summer by a question started by Sir W. Thomson, connected with Laplace's theory of the tides.

A comparison is instituted between the results of various processes employed to reduce the general linear differential equation of the second order to a non-linear equation of the first order. The relation between these equations seems to be most easily shown by the following obvious process, which I lit upon while seeking to integrate the reduced equation by finding how the arbitrary constant ought to be involved in its integral.

Let $u$ and $v$ be any functions of $x$,

$$\xi = \frac{A\dfrac{du}{dx} + B\dfrac{dv}{dx}}{Au + Bv} = \frac{u' + Cv'}{u + Cv} \quad . \quad . \quad . \quad . \quad . \quad (1),$$

where B and A, and therefore their ratio C, are arbitrary constants. The elimination of C from (1) must of course give a differential equation of the first order in $\xi$.

We have

$$\xi' = \frac{u'' + Cv''}{u + Cv} - \left(\frac{u' + Cv'}{u + Cv}\right)^2 .$$

Now we have, by adding and subtracting multiples of (1), &c.,

$$\xi' = \frac{u'' + Pu' + Qu + C(v'' + Pv' + Qv)}{u + Cv} - \left(\frac{u' + Cv'}{u + Cv}\right)^2 - P\xi - Q ;$$

whence, if $u$ and $v$ are independent integrals of the equation

$$y'' + Py' + Qy = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2),$$

we have the required equation

$$\xi' + \xi^2 + P\xi + Q = 0$$

and the process above shows why it takes this particular form.

But (2) gives

$$y = Au + Bv$$

as the complete integral, so we see that

$$\frac{y'}{y} = \xi.$$

Various classes of cases in which this form is integrable are given, of which the following is one:—

Let $\xi = \eta\sqrt{Q}$, then the equation becomes integrable in the form

$$\frac{\eta'}{\eta^2 + m\eta + 1} + \sqrt{Q} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3),$$

provided

$$P\sqrt{Q} + \tfrac{1}{2}\frac{Q'}{\sqrt{Q}} = mQ,$$

$$\textit{i.e.,} \qquad \frac{\epsilon^{-\int P dx}}{\sqrt{Q}} = -m\int \epsilon^{-\int P dx}\, dx.$$

The next subject treated is the effect of the alteration of sign of P or Q in (2). This is illustrated by the equation

$$y'' \pm xy' \pm y = 0,$$

which is integrable or at least reducible to quadratures for any of the four combinations of sign.

The always integrable case where

$$P = (C - x)\,Q$$

is next examined.

Another portion of the investigation deals with certain infinite but convergent series, whose sums can always be expressed in terms of the integral of a linear differential equation of the second order.

Consider, for instance, the expansion

$$\epsilon^{px + \frac{1}{x}} = \left(1 + px + \ldots + \frac{p^n x^n}{\underline{|n}} + \ldots\right) \times \left(1 + \frac{1}{x} + \ldots + \frac{1}{x^n \underline{|n}} + \ldots\right) (4)$$

$$= \Sigma\, P_n x^n, \text{ suppose.}$$

Obviously we have

$$P_n = p^n P_{-n} = \frac{p^n}{\underline{|n}} + \frac{p^{n+1}}{\underline{|1}\,\underline{|n+1}} + \frac{p^{n+2}}{\underline{|2}\,\underline{|n+2}} + \quad . \quad . \quad . \quad .$$

From this at once

$$\frac{dP_n}{dp} = P_{n-1}, \text{ whence } P_n = \left(\int dp\right)^n P_0 \quad . \quad . \quad (5).$$

Also

$$\frac{d}{dp}\left(\frac{P_n}{p^n}\right)=\frac{P_{n+1}}{p^{n+1}}\,,\text{ whence } P_n=p^n\left(\frac{d}{dp}\right)^n P_0 \quad . \quad . \quad (6).$$

Eliminating $P_n$ between (5) and (6), we obtain

$$P_0=\left(\frac{d}{dp}\right)^n p^n\left(\frac{d}{dp}\right)^n P_0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (7).$$

This equation is thus true for all positive integral values of $n$, and its form at once shows that it is true for negative integral values also. It is very singular that such a series of equations of all orders should have a common solution. But it depends upon the fact, which I do not recollect having seen in print, that

$$\left(\frac{d}{dx}x\frac{d}{dx}\right)^n=\left(\frac{d}{dx}\right)^n x^n\left(\frac{d}{dx}\right)^n.$$

This can be verified at once by applying it to $x^m$; as can also the companion formula

$$\left(x\frac{d}{dx}x\right)^n=x^n\left(\frac{d}{dx}\right)^n x^n.$$

Suppose we had, instead of (5) and (6),

$$\frac{dQ_n}{dq}=Q_{n+1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5^1),$$

$$\frac{d}{dq}(q^nQ_n)=q^{n-1}Q_{n-1} \quad . \quad . \quad . \quad . \quad . \quad (6^1),$$

we should find the *same equation* (7) for $Q_0$ as for $P_0$. In fact, as is easily seen,

$$Q_n=P_n.$$

Other pairs which alike give the equation

$$R_0=\left(\frac{d}{dr}\right)^n r^{-n}\left(\frac{d}{dr}\right)^n R_0 \quad . \quad . \quad . \quad . \quad . \quad (7^1)$$

are

$$\frac{dR_n}{dr}=R_{n+1}\,,\quad \frac{d}{dr}\left(\frac{R_n}{r^n}\right)=\frac{R_{n-1}}{r^{n-1}}$$

and

$$\frac{dS_n}{ds}=S_{n-1}\,,\quad \frac{d}{ds}(s^nS_n)=s^{n+1}S_{n+1}.$$

We thus get the two distinct particular integrals of each of the corresponding differential equations.

More generally,

$$\left(\frac{d}{dp}\right)^{\nu} P_n = P_{n-\nu},$$

and

$$\frac{P_n}{p^n} = \left(\frac{d}{dp}\right)^{\nu} \frac{P_{n-\nu}}{p^{n-\nu}};$$

whence

$$P_{n-\nu} = \left(\frac{d}{dp}\right)^{\nu} p^n \left(\frac{d}{dp}\right)^{\nu} \frac{P_{n-\nu}}{p^{n-\nu}}.$$

Changing $n-\nu$ to $m$, this becomes

$$P_m = \left(\frac{d}{dp}\right)^{-m} \left\{ \left(\frac{d}{dp}\right)^{n} p^n \left(\frac{d}{dp}\right)^{n} \right\} \left(\frac{d}{dp}\right)^{-m} \frac{P_m}{p^m},$$

which, when $m=0$, agrees with (7). Here $n$ may have any positive integral value not less than $m$. When we write $n=m$ we have merely a truism. If we put $n=m+1$, we arrive at the same result as we should have obtained directly from the *first* forms of the equations (5) and (6). All these series satisfy differential equations of the form

$$x\frac{d^2y}{dx^2} - (n-1)\frac{dy}{dx} = y.$$

Corresponding properties are easily proved for the series forming the co-efficients of the various powers of $x$ in the expansions of expressions like

$$\epsilon^{px^m + \frac{1}{x^n}}, \quad \epsilon^{px + \frac{\sqrt{-1}}{x}}, \text{ \&c., \&c.}$$

It is easily seen that what has been called $P_0$ above is the infinite series

$$P_0 = 1 + \frac{p}{1^2} + \frac{p^2}{1^2 . 2^2} + \frac{p^3}{1^2 . 2^2 . 3^2} + \ldots = f(p) \quad . \quad . \quad (8),$$

and that quite generally if

$$\Pi_m = 1 + \frac{p}{1^m} + \frac{p^2}{1^m . 2^m} + \frac{p^3}{1^m . 2^m . 3^m} + \text{\&c.}$$

we have

$$\Pi_m = \left(\frac{d}{dp}\right)^{n} \left(p^n \left(\frac{d}{dp}\right)^{n}\right)^{m-1} \Pi_m$$

whatever positive integer be represented by $n$. Of this the simplest case is $\Pi_1 = \epsilon^p$, where of course

$$\left(\frac{d}{dp}\right)^n \Pi_1 = \Pi_1 .$$

Again, just as the solution of this equation has the property

$$\epsilon^p \, \epsilon^q = \epsilon^{p+q},$$

so it is easy to see that we have in (8)

$$f(p) \; f(q) = f(\overline{p+q}),$$

where the bracket over $p+q$ is employed to indicate that in the expansion we must *square* the numerical co-efficients of each term of a power of this binomial, *i.e.*,

$$\begin{aligned} \overline{p+q} &= p+q, \\ \overline{p+q}^2 &= p^2+q^2+2^2pq, \\ \overline{p+q}^3 &= p^3+q^3+3^2(p^2q+pq^2), \\ \overline{p+q}^4 &= p^4+q^4+4^2(p^3q+pq^3)+6^2p^2q^2, \\ &\text{\&c.,} \qquad\qquad \text{\&c.,} \end{aligned}$$

and a similar property, though of course involving higher powers of the co-efficients, holds for each of the functions $\Pi_m$ above.

For the product of any two similar expansions (with different variables) is easily seen to have all its numerical co-efficients raised to any given power when those of the separate expansions are so raised.

The paper contains also an account of various attempts to solve the general equation of the second degree, of which the following may be noted.

*a.* Transform to

$$\frac{d^2y}{dx^2} - \mathrm{X}y = 0,$$

and evaluate

$$\iint \frac{1}{y}\frac{d^2y}{dx^2}\,dx^2$$

*at once*, just as

$$\int \frac{1}{y}\frac{dy}{dx}\,dx$$

is evaluated. The difficulty is reduced to finding the value of

$$\iint \left(\frac{dy}{dx}\right)^2 dx^2,$$

where a *single* operation is to be effected.

*b.* Transform to

$$\frac{d\xi}{dx} + \xi^2 = X,$$

and express this by the help of an auxiliary operation in terms of a merely artificial quantity $z$, so that

$$\frac{d\xi}{dx} + \xi^2 = \epsilon^{x\frac{d}{dz}} Z$$

so that all equations of the kind considered can be reduced to the very simple form

$$\frac{d\xi}{dx} + \xi^2 = A\epsilon^{ax}$$

If this were integrated, the only remaining difficulty would lie in the separation of symbols from the quantities they operate upon.

5. On Two-dimensional Motion of mutually influencing Vortex-columns, and on Two-dimensional Approximately Circular Motion of a Liquid. By Sir W. Thomson.

*Monday, 17th January 1876.*

The Right Rev. Bishop COTTERILL, Vice-President, in the Chair.

The following Communication was read:—

## On the Origin of Language—Max-Müller, Whitney. By Professor Blackie.

Professor Blackie stated that though the origin of language might be considered by some more a metaphysical than a philological question, it was yet so closely connected with philology, that whatever opinions a philologer held on this question could not fail to exercise a strong secret influence on his philological procedure. The primary elements out of which language grew were admitted by all to be three, viz., cries or interjectional exclamation, mimetic reproduction of audible sounds, technically but stupidly called onomatopœia, and gesture. But while agreeing on this threefold basis the most distinguished writers on this subject, such as Max-Müller, Wedgwood, Whitney, Bleek, Schleicher, and Steinthal, disagreed fundamentally, or at least seemed to be at daggers drawing, with regard to the course which language pursued in its further develop-

ment. On the one hand, Müller, who had had a great influence in guiding public opinion in this region, stoutly asserted that out of the three primal elements, as from a root, no further growth of what we called human language, for reasonable social purposes, could take place; while, on the other hand, Wedgwood—who in this whole matter had, in his opinion, received scant recognition from the scholars of his own country—as stoutly maintained that from these three elements, as their natural root, the whole organism of the beautiful growth of language, stem, leaf, and fruit, could be satisfactorily explained. After carefully studying the arguments of the learned Oxford Professor, he was of opinion that Wedgwood was in the main right. To this conclusion he came from a course of independent investigation some years ago, and when, curiously enough, he had only used Wedgwood's dictionary for occasional consultation, without having read and pondered the discourse prefixed to the last edition of that work. The grounds of his opposition to Müller were stated to be simply these:—While in perfect agreement with him that roots significant of ideas are the ultimate facts in the analysis of languages as we now have them, and believing also that such conceptional roots are the natural and necessary expression of reason in a reasoning animal, and explicable only on the supposition of an indwelling plastic reason in man, I am at the same time unable to see why this plastic reason in the formative process of language-making should not have used the materials so amply supplied by the interjectional and mimetic elements of the simplest germs of speech. The interjectional element, I call the principle of significant vocal response; and the onomatopoetic principle, I call the mimetic, dramatic, or pictorial element in language; and I am prepared to show that, even under the many defacements and obliterations which spoken words, like old sixpences or wave-worn pebbles, suffer from the tear and wear of time, they yet show in hundreds of cases on their face the manifest superscription of their mimetic origin. For we must take note, first, that not only pigs and cuckoos, cats, curs, and crows, but all nature, is full of sounds, and that there is no absolute silence anywhere but in death; and, further, that not only an immense variety of sounds can be approximately expressed by imitation in articulated vocal breath, but that by an easy transference the impressions of the other senses can be analogically expressed in the flexible material of significant

sound. It is also not only an easy but a natural and necessary process of the human mind in the formation of general concepts to use the material presented by mere sensuous impressions; and thus, while we expressly deny the perverse doctrine of the sensationists that mind can be explained from sense, or the imperial unity of thought be generated from a multiplicity of external impressions, we can see no difficulty in deriving such general concepts as *character* and *type*, for instance, from the roots χαράσσω and τύπτω, which originally were mere mimetic reproductions of an external sound, as in our word *scratch*. Language, therefore, was formed by the gradual extension of words originally expressing sensations and feelings to intellectual purposes; and there is nothing ignoble in this, for the mind uses the materials supplied by sense just as the architect uses the stones dug by the quarryman or the lime carried by the hodman. Neither can one at all see the logical justice of Max-Müller when he exposes the historical falsehood of some of Wedgwood's onomatopoetic etymologies; for the erroneous application of a principle does not in the least imply that the principle itself is erroneous; and, besides, the oldest roots to which certain very recent forms of Romanesque words may be traced back in Sanscrit can be shown in not a few cases to have been the product of that very mimetic process which Müller so persistently ignores. But while Müller seems actuated by some strange prejudice in his stout determination to make no use of the onomatopoetic element so thickly strewn in language, Professor Whitney has introduced no little confusion into this matter by talking of language as an *institution*, and reviving the doctrine of the old Greek Sophists, that language is θέσει, not φύσει. Every simile limps; but if we must have similes, it is far nearer the truth to talk of language as a growth and a living organism than to call it an institution. In some sense language is certainly a growth; in no sense is it an institution. Institutions like the Sabbath, for instance, are then creatures of positive law; but language is a direct efflux of plastic reason, and no more an institution than the song of the nightingale or a sonata of Beethoven. As to the connection between Darwinism and the origin of language, while the Darwinian philologers, with Schleicher at their head, will no doubt find a special delight in tracing the splendid roll of a Platonic period from the grumph of

the primeval pig, and the mew of the pre-Adamitic kitten, those who with me look on Darwinism as a mere pleasant conceit of men besotted in the one-sided study of physical science, can, so far as philological conclusions are concerned, leave the conceit to shift for itself, being firmly convinced that whenever reason does show itself whether on the original appearance of man or at some after-stage of his development, it appears as a force altogether different from, and in some of its functions, as Professor Ferrier wisely maintained, essentially contradictory of, and antagonistic to, every kind and degree of mere sensation; and in this character brings forth language as the natural manifestation and organised body of itself.

*Monday, 7th February* 1876.

SIR WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. Note on Certain Formulæ in the Calculus of Operations. By Professor Stokes, Hon. F.R.S.E. (In a letter to Professor Tait.)

"*January* 14*th*, 1876.

"Formulæ like those you sent me* are readily *suggested* by supposing the function operated on to be of the form $\Sigma Ax^a$, or say, for shortness, $x^a$, with the understanding that no transformations are to be made which are not equally valid for $\Sigma Ax^a$.

Thus

$$\left(\frac{d}{dx}x\frac{d}{dx}\right)^n x^a = a^2(a-1)^2 \;.\;.\;.\; (a-n+1)^2 x^{a-n}$$

$$= a\,(a-1) \;.\;.\;.\; (a-n+1)\left(\frac{d}{dx}\right)^n x^a$$

$$= \left(\frac{d}{dx}\right)^n x^n \left(\frac{d}{dx}\right)^n x^a\,;$$

and

$$\left(x\frac{d}{dx}x\right)^n x^a = (a+1)(a+2) \;.\;.\;.\; (a+n)x^{a+n}$$

$$= (a+n)(a+n-1) \;.\;.\;.\; (a+1)x^{a+n}$$

$$= x^n\left(\frac{d}{dx}\right)^n x^n x^a\,.$$

* See *ante*, p. 95.

The direct transformation may readily be effected by noticing, in the first instance, that any two operations of the form

$$x^{-m+1}\frac{d}{dx}x^m$$

are convertible. We find, in fact,

$$x^{-m+1}\frac{d}{dx}x \quad . \quad x^{-n+1}\frac{d}{dx}x^n = x^2\left(\frac{d}{dx}\right)^2 + (m+n+1)x\frac{d}{dx} + mn,$$

into which $m$ and $n$ enter symmetrically.

Replacing the operations in the left hand member of the first formula by convertible operations, which will be separated by points, we find

$$\frac{d}{dx}x\frac{d}{dx} = x^{-1}\times x\frac{d}{dx} \; . \; x\frac{d}{dx},$$

$$\frac{d}{dx}x\frac{d}{dx}x^{-1} = x^{-2}\times x^2\frac{d}{dx}x^{-1} \; . \; x^2\frac{d}{dx}x^{-1},$$

and so on. Hence,

$$\left(\frac{d}{dx}x\frac{d}{dx}\right)^n = x^{-n}\left(x^n\frac{d}{dx}x^{-n+1}\right)^2\left(x^{n-1}\frac{d}{dx}x^{-n+2}\right)^2\cdots\left(x\frac{d}{dx}\right)^2,$$

$$= x^{-n}\left\{x^n\frac{d}{dx}x^{-n+1} \; . \; x^{n-1}\frac{d}{dx}x^{-n+2} \; \ldots \; x\frac{d}{dx}\right\}^2,$$

$$= x^{-n}\left\{x^n\left(\frac{d}{dx}\right)^n\right\}^2 = \left(\frac{d}{dx}\right)^n x^n\left(\frac{d}{dx}\right)^n.$$

Again,

$$x\frac{d}{dx}x = x\times\frac{d}{dx}x,$$

$$x\frac{d}{dx}x^2 = x^2\times x^{-1}\frac{d}{dx}x^2,$$

and so on. Hence

$$\left(x\frac{d}{dx}x\right)^n = x^n\times x^{-n+1}\frac{d}{dx}x^n \; . \; x^{-n+2}\frac{d}{dx}x^{n-1}\cdots\frac{d}{dx}x,$$

$$= x^n\times\frac{d}{dx}x \; . \; x^{-1}\frac{d}{dx}x^2 \ldots x^{-n+1}\frac{d}{dx}x^n,$$

$$= x^n\left(\frac{d}{dx}\right)^n x^n.$$"

2. A Further Contribution to the Placentation of the Cetacea (*Monodon Monoceros*). By Professor Turner.

In the year 1871, I read before this Society a memoir on the Gravid Uterus of *Orca gladiator*, in which I discussed the placentation of the *Cetacea*. This memoir was published in the Transactions for that year. On the present occasion I purpose describing the placenta in a Cetacean genus in which it has not hitherto been examined.

In the month of December 1875 I received, through the intermediation of my friend Mr C. W. Peach, from Mr John Maclauchlan, the chief Librarian and Curator to the Free Library, Dundee, a cask containing the gravid uterus of a Narwhal (*Monodon Monoceros*), which had been procured by the captain of the Dundee whaling steamer "Erik." The uterus had been preserved in strong brine, and was in good condition for anatomical examination.

The uterus was two-horned, and contained a fœtus 5 feet 5 inches long in the left cornu. The gravid horn measured 7 feet 4 inches along its great curvature; the non-gravid, 4 feet. The girth of the gravid horn, at its thickest part, was 4 feet 4 inches. The length of the corpus uteri was 1 foot; that of the vagina, 1 foot 8 inches. The os was occluded by an extremely viscid mucus.

The uterine cornua were opened into by a longitudinal incision along the greater curvatures. The uterine wall was comparatively thin, and the chorion was closely adherent to its mucous lining. By an incision through the chorion, along the greater curvature of the gravid horn, the sac of the amnion was opened into and the fœtus exposed. The fœtus lay with its back in relation to the greater curvature of the cornu, its belly to the lesser curvature, its head close to the corpus uteri; whilst its caudal end was directed to the narrow end of the horn, but did not reach to within two feet of the Fallopian tube. The tail was curved forwards under the hinder part of the ventral surface of the fœtus. The pectoral flipper was directed backwards parallel to the long axis of the body. The umbilical cord was 3 feet long, spirally twisted, and bifurcating where it reached the sac of the allantois. The amnion formed an immense bag, which reached to 5 inches from the free end of the gravid horn of the chorion, but it did not extend into that part of

the chorion which occupied the non-gravid horn. The amnion was closely adherent to the greater part of the chorion in the gravid cornu; but that portion of the chorion which was attached to the mucosa lining the lesser curvature of the cornu, and which lay opposite the abdominal aspect of the fœtus, was in relation to the wall of the sac of the allantois. The allantois formed a large funnel-shaped bag at the place of bifurcation of the cord. It was prolonged along the concavity of the chorion to within 2 inches of its free end in the gravid horn, and to within 9 inches of the free end of the prolongation of the chorion into the non-gravid horn. The length of the sac of the allantois was therefore much greater than that of the amnion, though its capacity was much less. The allantois was prolonged as a slender tubular urachus into the umbilical cord, which also contained two large arteries and two veins. The amnion investing the cord had numerous brownish corpuscles, resembling those I have described in *Orca gladiator*, projecting from it; and similar corpuscles were scattered over that part of the amnion which was in apposition with the wall of the sac of the allantois, and a few were seen on the amnion beyond the border of the allantois. In addition to these brown corpuscles, numerous other bodies of a dull white appearance were found. Sometimes these were slender rods, from $\frac{1}{10}$th to $\frac{4}{10}$th inch long, arranged end to end like the links of a chain, at other times they were globular, like minute shot. The rods were most numerous on the abdominal half of the cord, whilst the globules were most numerous at and near its bifurcation. The surface of the amnion adjacent to the cord had a few of these globules scattered over it. These white bodies were covered by the smooth amnion, which with a little care could be stripped off as a distinct pellucid membrane. They consisted of crowds of squamous epithelial cells, so that in structure they resembled the whitish bodies which are so abundantly developed in connection with the amnion of the cow. Between 3 and 4 inches of the abdominal end of the cord was covered with cuticle, which had the purplish-grey colour of the cuticular investment of the adjacent surface of the wall of the belly.

The two uterine cornua became continuous with each other through the corpus uteri, and were partially separated by an

imperfect septum, which projected from the inferior wall. Owing to the great distension of the left cornu, this septum was pushed to the right, so that the os uteri opened directly into only the gravid horn. The chorion extended from the end of the gravid to that of the non-gravid cornu. As it passed through the corpus uteri it was somewhat constricted by the projecting septum. In the whole length of the non-gravid horn, and at the free end of the gravid horn, the chorion was raised into strong longitudinal folds, which corresponded in reverse order with a similar series of folds of the uterine mucosa radiating from the orifices of the Fallopian tube. At the os uteri the mucosa was raised into strong folds, which radiated into the gravid chorion for a considerable distance, and in some parts of their extent projected as much as 3 inches from the general plane of the mucosa, though at the os they had not more than one half that projection. The chorion in apposition with this part of the mucosa was also folded. In the gravid horn opposite the fœtus, where the expansion both of chorion and uterus was the greatest, the folds were not present. Except in a few localities, to be immediately specified, the whole of the extensive surface of the chorion was so covered with vascular villi that, to the naked eye at least, no non-villous intervals could be recognised. The chorion was adherent to the uterine mucosa, so that gentle traction was needed to draw them asunder; and, as the one was peeled off the other, the villi of the chorion were seen to be drawn out of multitudes of crypts opening on the free surface of the mucosa.

The chorion, which lay opposite the os uteri and the immediately surrounding mucous membrane, was for the most part not villous, but presented a smooth, feebly vascular appearance, which contrasted strongly with the adjacent villous chorion. This smooth spot was irregular in form, measured 6 inches by 4 inches, and from it narrow bands of smooth chorion radiated outwards for from 2 to 3 inches between the villous covered folds of the chorion. It was similar to, but much larger, than the corresponding spot in *Orca* and the Mare. Small isolated patches of villi were scattered irregularly over the surface of this smooth spot. The inner surface of the chorion at the bare patch was lined by the amnion and not by the allantois. Three inches from this large spot a bare patch, 1 inch by inch, was completely surrounded by villous chorion.

The uterine mucosa opposite these smooth portions of the chorion was smooth and free from crypts, except where the isolated patches of villi were in apposition with it. Radiating for about 1 inch from the pole of the chorion in the gravid horn were narrow non-villous bands of the chorion separated by intermediate villous surfaces. These bands corresponded to folds of the mucosa free from crypts, which radiated from the orifice of the Fallopian tube, and were continuous with the longitudinal folds of mucosa in that tube. In the non-gravid horn the chorion was devoid of villi for about 5 inches from its free end, and for even a greater distance the villi were irregularly scattered so that well-defined smooth patches could be traced as far as 10 or 12 inches from the pole. On some parts of the chorion pedunculated hydatid dilatations of the villi, about the size of small peas, were irregularly scattered.

When examined microscopically, the villi were seen to be arranged in tufts, which varied in size and in the number of villi. Some tufts had not more than two or three villi, but more usually numbers were collected together, though occasionally short single villi arose from the chorion in the intervals between the tufts. The villi had as a rule a club-shaped form, but some divided into filiform branches. They were highly vascular, and a beautiful extra-villous layer of capillaries was distibuted, as in *Orca*, beneath the free surface of the chorion.

The free surface of the uterine mucosa had, as in *Orca*, a delicate reticulated appearance, and was pitted with multitudes of recesses and furrows, which again were subdivided into innumerable crypts. In the polar regions of the cornua and in the corpus uteri the mucosa was more spongy and succulent than in the greatly distended part of the gravid horn, in which the mucosa was obviously more stretched, so that the pits and furrows were almost obliterated, and the crypts opened on the general plane of the mucosa. In their general arrangement, and in the vascularity of their walls, the crypts in the Narwhal resembled so closely the corresponding structures in *Orca*, that I need not give a special description. The layer of cells which lined them was a well-defined cylindrical epithelium, many of the cells of which, however, were so swollen that the breadth almost equalled the length.

Scattered over the surface of the mucosa in the more distended part of the cornu were numerous smooth, depressed, circular or ovoid spots, the largest of which was not more than $\frac{2}{10}$th inch in diameter, though as a rule they were less than $\frac{1}{10}$th inch; so that to the naked eye they were apt to escape observation. Each spot was surrounded by a minute fold of the mucosa, sub-divided into crypts. On an average from twenty-five to thirty of these spots were found in each square inch. They resembled the smooth depressed spots described by myself* and some other anatomists in the uterine mucosa of the gravid pig. On the surface of the chorion adapted to this part of the mucosa, occasional smooth patches from $\frac{1}{10}$th to $\frac{2}{10}$th inch in diameter were seen surrounded by villous tufts which were in apposition with the smooth depressed spots on the mucosa, but they had not the definite stellate form which one sees on the chorion of the pig. The extra-villous layer of capillaries ramified beneath these non-villous spots of the chorion. In the succulent parts of the mucosa the smooth depressed spots could not be seen with the naked eye, but only after a careful search with a pocket lens were they found at the bottom of some of the trenches or pits in the membrane.

From the general resemblance between these spots and those met with in the uterine mucosa of the pig, one was naturally inclined to think that they would have a relation to the mouths of the utricular glands. I proceeded, therefore, to examine the glandular layer of the mucosa. The glands were very numerous, and branched repeatedly. Many of the branches formed short diverticula, others were much longer; sometimes they were tortuous, at others a considerable length of straight gland tube could be seen. In the deeper part of the mucosa the glands lay almost parallel to the surface; but as they approached the crypts, they were directed more obliquely, so as to be frequently divided in vertical sections. The glands were subjacent not only to the crypts, but to the smooth depressed spots, numerous examples of which I carefully examined. In one instance, I saw a tube lined by epithelium lying obliquely beneath the membrane of a spot, and opening near the middle by a distinct orifice bounded by a crescentic fold of the membrane. In

* "Journal of Anatomy and Physiology," Oct. 1875. Also Lectures on the Comparative Anatomy of the Placenta, Edinburgh, 1876.

a second instance, the end of a gland passed from under cover of the surrounding crypts, and then seemed to open by an obliquely directed mouth near the free edge of the spot. But upwards of thirty other spots examined with equal care gave me no evidence of gland mouths opening on them. Hence it would appear that these smooth surfaces on the mucosa are by no means necessarily associated with the mouths of the utricular glands, and one is disposed to conclude that the gland orifices are usually concealed amongst the crypt-like foldings of the mucosa. The very much greater number of the crypts than of the stems of the glands, negatives the idea of the crypts being merely dilatations of the mouths of the glands, so that in the Narwhal, as in *Orca*, the Pig, and Mare, the crypts are to be regarded as interglandular in position and produced by a hypertrophy and folding of the mucosa.

Through the kindness of my friend Dr Allen Thomson, I have had the opportunity of examining a portion of the gravid uterus and chorion of a Narwhal, the fœtus in which measured only 3¼ inches long. The free surface of the mucosa was gently undulating and traversed by shallow furrows, but no definite crypts could be seen. The gland-tubes were remarkably numerous, tortuous, and branching. I made a comparative measurement of their size, with that of the glands in my much more developed specimen, and found them to have only one-half the transverse diameter. The gland-stems inclined obliquely to the surface of the mucosa on which their orifices could occasionally be seen. The free surface of the chorion was not villous, but traversed by faint ridges, which without doubt fitted into the shallow furrows of the mucosa. Patches of epithelium-cells could be seen covering the surface of the chorion. It is clear therefore that in the Narwhal, as I have elsewhere described in the pig, the villi do not form on the surface of the chorion, nor the crypts on the surface of the mucosa, until the embryo has reached a stage of development in which its body, though small, has assumed a form which enables its ordinal characters to be recognised.

When I published my memoir on the placentation of *Orca*, I was under the impression that the crypts were lined by a pavement epithelium, and was not disposed to regard the crypts in the mucosa of that cetacean as secreting organs; but a re-examination

of the epithelium-cells lining its crypts has convinced me, that they are not a pavement epithelium, in the sense of being squamous cells, but have an intermediate or transitional form between the columnar epithelium and the tesselated epithelium. In the Narwhal, again, the cells are cylindrical, as in so many mammals. so that I believe the Cetacea to offer no exception to the view that these cells are a secreting epithelium, and they doubtless elaborate a secretion for the nourishment of the fœtus. From the fact that the utricular glands had a much greater calibre in my specimen than in the one belonging to Dr Thomson, one may infer that even after the crypts are fully developed the glands still play a part in fœtal nutrition.

The fœtus in my specimen had an almost uniformly purplish-grey colour, but with a patch of yellowish-white on the belly near the anus. The snout was rounded; the fissure of the mouth 1½ inch long; eye-slit 4½ inches behind the snout, and surrounded by a faint circle; ear orifice very minute, 3 inches behind the eye-slit; blow holes above and a little anterior to eye slit. Length of flipper 7 inches, its anterior edge 12 inches behind the snout. Funis was attached to the belly about midway between the anterior and posterior end of the fœtus. A low, but distinct dorsal ridge, the rudiment of a dorsal fin, commenced a little in front of a point midway between tip of snout and end of tail, and extended backwards for between 10 and 11 inches along the middle line of back. It had a lighter greyish tint than the surrounding skin. Breadth of tail was 15½ inches. In a profile view of the fœtus a slight depression in the contour of the top of the head was seen in the region of the blow-holes. The fœtus *in utero* differed therefore very materially in colour from such a half-grown specimen as Dr Fleming described*, in which the upper part of the body was a dusky black, the belly white, and numerous oblong spots extended horizontally along the sides; still more did it differ from old specimens, which have a whitish marbled colour. The presence of a dorsal ridge is also of interest, as the Narwhal is described as without a dorsal fin.

I availed myself of the opportunity of examining the dentition of the fœtus. On May 20th, 1872, I described to the Society the

* Memoirs Wernerian Socy., 1811.

dentition of a fœtal Narwhal $7\frac{1}{4}$ inches long, in which I found two dental papillæ developed in the gum, on each side of the upper jaw, but the early stage of development of the fœtus did not permit me to say whether the anterior or posterior denticle would have been the one to become the maxillary tusk, though I thought it probable that the more anterior would become the tusk.

In this much larger fœtus the superior maxilla was $8\frac{1}{2}$ inches long. At the anterior end of each of these bones were two well marked sockets, one opening immediately behind the other. The anterior socket contained a cylindriform rudimentary tusk. The posterior socket contained an aborted tooth $\frac{1}{2}$ inch long, and $\frac{2}{10}$ inch in its widest diameter. The hinder half of the aborted tooth was attenuated, and had several short irregular processes projecting from it; the anterior half was smooth and rounded. This tooth was inclosed in a distinct sac, formed of fibrous tissue, which, like the sac of the rudimentary tusk, was firmly united to the fibrous tissue of the gum. There can be no doubt, therefore, that I was right in my conjecture that the more anterior dental papilla becomes the tusk of the Narwhal.

## 3. Observations on the Zodiacal Light. By C. Michie Smith. Communicated by Professor Tait.

While engaged in cable work in the West Indies, I had, during the winter and early spring of 1875, a number of very favourable opportunities of examining the zodiacal light.

Before leaving this country I had, under the advice of Professor Tait, and with a note of recommendation from Professor Jenkin, applied to the Royal Society for the loan of a spectroscope, to make observations with during the voyage; but unfortunately I was unable to obtain one, and so had to content myself with a small pocket-spectroscope. On the outward voyage I did not notice the light at all till we got well to the south, near Cayenne, on the 8th of January; and, owing to the very bad weather we had about that time, I was not able to make any satisfactory observations till we were again somewhat farther north. The general appearance of the zodiacal light has been so often described, more or less faithfully, that I need not attempt any description of it here. I wish,

however, to mention one feature with which I was much struck, and which I have never seen remarked on—namely, that, on watching the western sky from sunset onwards, it is impossible to tell when the diffused sunlight ends and the zodiacal light begins till it becomes so dark that the form of the latter can be traced to a considerable altitude, when it is seen to be longer and narrower, and inclined to the vertical at a considerable angle. I am strongly inclined to believe that near the sun it is very much wider than at some distance from it, for I have very good reason to think that what by people generally would be taken as simply the last of the sunset glow is really due to the zodiacal light. This part, of course, can only be seen in places where the twilight is very short. The best time for making observations I found to be about two hours after sunset, when all traces of twilight had certainly disappeared, and consequently all risk of confusion with it was gone.

On January 31st, in lat. 8° N., long. 56° W., the light was very bright, and I made some spectroscopic observations. At two hours after sunset the light was visible for 90° from the horizon, and so bright was it towards the horizon that I was able to get a distinct spectrum. I first opened the slit very wide, when I observed a broad strip of light, nebulous at the extremities, with a distinct reddish tinge at the one end; then, by gradually closing the slit, I obtained a narrower but tolerably pure continuous spectrum, in which I could *distinctly* see reddish, orange, and greenish-blue, and on making comparison with the spectrum of a lamp (placed at the far end of the ship so as not to dazzle my eyes), I estimated that the spectrum extended from the red to past the position occupied by the F line in the solar spectrum. A large number of observations taken on other nights, whenever the circumstances were favourable, entirely confirmed these first observations. On several nights, and especially on February 27th, when off Ponce, in the Island of Puerto Rico, I observed the spectrum from a short time after sunset till long after the last traces of twilight had disappeared, but no change was noticeable after the spectrum had become so faint that the Fraunhofer lines could not be distinguished, except in the brightness of the spectrum, as I was still able to see colour distinctly, but no traces of any bright lines.

On February 26th and 27th I took a number of sextant measure-

ments of the zodiacal light. These are, of course, only approximate, as the light has no definite boundary-lines, but gradually fades off at the borders. The measurements were made by the help of stars, as it was quite impossible to measure the light itself. On the 26th sunset was at about six o'clock, the light was very bright for 30° from the horizon, having at the horizon a breadth of about 25°, and at an altitude of 30° a breadth of about 20°. At 9 P.M. the light could be traced quite round to the eastern horizon, a phenomenon which I observed on several other occasions. At 10 P.M. the light was scarcely, if at all, visible. That this sudden disappearance was not caused by any change in the atmospheric conditions was clearly shown by the undiminished brightness of the stars. On February 27th the breadth at the horizon was 30°, while at an altitude of 30° it was only about 20°. The centre of the band passed a little to the south of the Pleiades. I endeavoured, by means of the sextant, to measure the inclination of the band to the vertical. For this purpose I chose two bright stars near the centre of the band—one at a considerable altitude, the other close to the horizon; I then measured the angle between these and the angle between the upper one and the horizon; these angles were respectively 55° 30′ and 51°, giving the value of 31° 30′ for the inclination of the centre of the band to the vertical. I had unfortunately no access to star-charts, else I could have fixed the direction more accurately; but even these rough observations confirm the ordinary statement that the direction is slightly inclined to the ecliptic.

The spectroscope used was one of Mr Ladd's admirable small direct-vision spectroscopes, with five prisms and a single lens. Behind the slit is a small round hole, through which the light may be made to enter by opening the slit very wide, and which is very convenient for examining monochromatic light. With this instrument it is easy to see a number of the Fraunhofer lines on a tolerably clear moonlight night. And such an instrument, though in some respects inferior to a simple large prism and slit, will, I believe, be found very suitable for work such as that described above, especially when it has to be carried on on board a ship much given to rolling.

### 4. Note on the Volcanoes of the Hawaiian Islands. By J. W. Nichol, F.R.A.S. Communicated by Professor Tait.

The late Transit of Venus Expedition gave the writer some opportunities of visiting several islands of the Hawaiian Archipelago, some details of which may prove interesting.

They form a group of islands about $10\frac{1}{2}$ hours west longitude from Greenwich, and about 20° to 22° north of the equator, and differ in size from 10 miles long by 6 or 7 broad, to 90 miles by 60, which are about the dimensions of the most easterly and largest, viz., Hawaii (the Owyhee of Cook). The general lie of the islands is from north-west to south-east, those in the east displaying the most recent traces of volcanic activity. In the older or western portion the main mountain ranges run in the same direction as the islands, rising in many places to a height of 3000 to 4000 feet, and having lateral ridges branching off at right angles, with an occasional crater of oval shape thrown up at a distance from them, and evidently of more recent origin.

The putting up of a meridian mark on one of these ridges in the island of Oahu was attended with some difficulty, the narrow space along which one had to ride, sometimes not more than a yard or two wide, with precipitous descents of 500 to 1000 feet on either side, not rendering it comfortable to any one with weak nerves.

The two most easterly islands, viz., Maui and Hawaii, although having their greatest length in the north-east and south-west directions, are composed only of mountains standing singly, and present no appearance of ranges. Maui, indeed, is nothing more than a couple of mountains joined by a very low neck of land, on the top of the eastermost of which (Mount Haleakala), at a height of 11,000 feet, is found one of the largest and most perfect extinct craters in the world, being some 10 miles in diameter, which unfortunately time would not admit of our visiting. The sides of Haleakala are not precipitous, and the general view from the sea is that of a huge hog-backed mountain. Traces of flows of lava so recent as to be quite black, and not covered with vegetation, are also seen coming down from what had been openings in its sides a short distance above the sea-level.

The island of Hawaii, or the most recent and easterly, is composed of four mountains—the Mauna Kea, 14,500; Mauna Loa, 13,800, Huallalei, 8000, and Kohala, about 5000 feet in height, with large valleys of 2000, 3000, to 6000 feet above the sea-level between. The slope of the mountains is usually gentle, and numerous small craters of 100 to 300 feet in height are found distributed on their sides, and also in the intermediate valleys. On the west side precipitous rocks face the sea, with a height of 3000 feet, which have valleys opening seawards that are almost inaccessible from the land side, owing to the precipitous character of their sides. It is on this island that the most recent displays of volcanic activity are seen, the country having been overrun in many places by lava flows, which have left large tracks quite useless for agricultural purposes. Earthquakes are common, and the summit crater of Mauna Loa, 13,800 feet up, is frequently, and that of Killauea (on a level plateau on the side of Mauna Loa, about 3000 feet above the sea) is almost always in a state of activity. The crater of Killauea is on the north-east of Hawaii, about 32 miles from the bay of Hilo, which is the most convenient starting-point for those wishing to visit the volcano. In the ride of 32 miles one has an ascent of some 3000 feet, but the ups and downs are so numerous that one can hardly detect it. During some portion of the way one passes through very dense tropical vegetation, palms, tree ferns, &c., with creepers and ferns clustering around, but for the most part the path lies over lava flows, so recent as to be almost devoid of vegetation, which render it so rugged as to compel one to walk his horse the greater part of the way. When approaching the volcano the visitor is at first struck by the sight of hundreds of steam jets rushing up in all directions, some of which are utilised as vapour baths, by putting a wooden box over them with a hole in the top large enough to admit the neck of the bather. On going some hundred yards further, an immense pit appears, at the further end of which during the day are seen large volumes of smoke, while at night a red flare is visible in the sky, with an occasional piece of white hot lava getting tossed up high enough to be seen above the edge of the inner crater. This large pit or outer crater is of oval shape, and some 3 miles long by $1\frac{1}{4}$ to $1\frac{1}{2}$ mile broad in the widest part. The sides are precipitous, and from 600 to 700

feet in height, being in many places divided into close parallel ridges, showing the height to which the lava had reached before breaking out at a lower level. There were two large sulphur beds at the side of the outer crater, about 2 miles distant from each other, and pieces of native sulphur could be picked up, each with a hole in its centre, showing that the vapour had solidified round the hole from which it had emerged. The floor of the outer crater was composed of black lava, several acres of which were covered over one night by the lava breaking out at a side of the lowermost edge of the outer crater, quite distant from the inner one.

Our first descent was made at night by means of some rustic staircases cut in the sides of the ridges, and assisted by whatever brushwood might be growing on the sides. At last our party were landed on a floor of black lava, all seamed with cracks and contorted into curious shapes, sometimes like a mass of cable ropes mingled together, at others showing large pudding-like excrescences, which are dangerous to walk upon, since they are simply large bubbles with a thin covering, to guard against which each visitor carries a thick stick wherewith to test the ground before him.

After walking for about half an hour on this black lava, and crossing innumerable cracks of from three inches to a foot in width, some of which showed a white line of fire about 6 feet beneath, the gradual ascent to the inner crater was reached. Its position in regard to the general form of the outer crater may be said to be in one of its foci, and its size, by estimation, about a ½ mile long by ¼ broad. The increasing glare and smoke now warned us of our proximity to the more active parts, while here and there on the outer side of the inner crater were some bright red streaks, which, on our closer approach, turned out to be red hot lava flowing through holes in the outer side of the inner crater on to the general surface of the outer one.

The ascent of some 70 feet to the top of the inner crater is a gradual one, and considerable detours had to be taken to get round the parts which were being overflowed. The lava of its sides was twisted about and broken up in a most ugly manner, besides being so rotten as to break away in flakes whenever a foot was put upon it. The greatest caution was needed to test the ground before treading on it, and frequent play had to be made with our thick

sticks. Tumbles were frequent, from which the writer escaped unharmed by having a thick pair of dogskin gloves on his hands. One of our party, however, a professor from Indiana, managed to fall into a crack up to his middle, and got his hands severely cut and burnt.

The view from the top, however, amply repaid the trouble. Four lakes of molten lava, the largest some 200 yards in length, and of kidney shape, and the others of smaller size, were seen in full activity. In the largest lake seven to eight fountains of white hot lava were playing up at once to a height of 30 to 40 feet, one sometimes stopping and another commencing at a different part of the side of the lake.

The lava in this lake was about 50 feet below the inner edge of the crater, and appeared to be slowly advancing toward the tunnels from which we had seen it issuing on the road up. The lakes were not at the same level, and you might see one brimful and another 60 to 70 yards off at a level of some 30 or 40 feet below it. On another of the lakes, about 50 yards wide, was a single fountain, bursting from a cavern in its side, and throwing lava half-way across its surface, while from the roof and sides of the cavern hung down lava stalactites.

After looking at this for some time, the claims of our injured friend became so strong, as to oblige us to take him back to the crater house, resolved next day to have a more deliberate inspection.

The next day proved wet, but the writer explored his way through the driving mist formed by the rain coming in contact with the heated lava, the only disagreeable incident being his getting to the leeward of a blow-hole, and having to run to get clear of the suffocating sulphur vapours. This blow-hole was about the size of a man's body, and as you went forward to it you heard a gurgling sound beneath. Smoke was coming out in considerable volumes, and on looking in, the sides were seen to enlarge beneath and be at a white heat.

Having at length got seated comfortably upon an upheaved block of lava about 20 feet above the larger lake, and 8 to 10 yards from its side, a new fountain sprung up suddenly from the side of the lake quite close at hand, which immediately forced a retreat to

a more respectful distance. About the same number of fountains continued to play up as on the preceding evening, and looked red by day. Daylight, however, drowned out the redness of the lakes as seen by night, and made them appear quite black.

After watching for a considerable time, a red hot crack was seen to start suddenly from one side of the lake to the other, then other cracks in different directions, and first one-half of the lake and then the other was covered with a fresh coating of red hot lava, the former tumbling out of sight as it got shrunk and cracked in cooling.

A curiosity called Pele's * hair is found round the sides of these lakes. This is composed of fine fibres of lava cooled, broken off from the molten liquid while being spouted up in the fountains, carried away by the wind, and lodged in the cracks around.

The summit crater of Mauna Loa, some 15 miles off, and 10,000 feet above Killauea, was in activity about a month previous to our visit to the island, but limited time prevented our seeing it. Some points of curiosity may be noted before ending.

1. The lakes are not at the same level, although quite close to each other.

2. The summit crater of Mauna Loa is 10,000 feet above Killauea, and frequently in violent eruption, while Killauea is comparatively undisturbed.

3. The outer crater of Killauea appeared to act as a receptacle for the lava, which, as soon as it arrived at a sufficient height, and got the assistance of an earthquake, broke through below and covered the country, sometimes running in a broad stream for 25 miles, and leaving an indication of the level which it had reached in form of a new ridge within the lip of the outer crater.

4. The necessity of an earthquake to enable it to break through is shown by the great difference of heights of the lava even within short distances.

5. The fountains were in every case playing round the *edges* of the lakes.

## 5. New General Formulæ for the Transformation of Infinite Series into continued Fractions. By Thomas Muir, M.A.

* The name of the Hawaiian Fire-Goddess.

## 6. Laboratory Notes. By Professor Tait.

### (*a*) On a Possible Influence of Magnetism on the Absorption of Light, and some correlated subjects.

Professor G. Forbes' paper, read at a late meeting of the Society, and some remarks made upon it by Professor Clerk-Maxwell, have once more recalled to me an experiment which I tried for the first time rather more than twenty years ago, in Queen's College, Belfast. I have since that time tried it again and again, whenever I succeeded in getting improved diamagnetics, a more powerful field of magnetic force, or a more powerful spectroscope. Hitherto it has led to no result, but it cannot yet be said to have been fairly tried. I mention it now because I may thus possibly be enabled to get a medium thoroughly suitable for a proper trial.

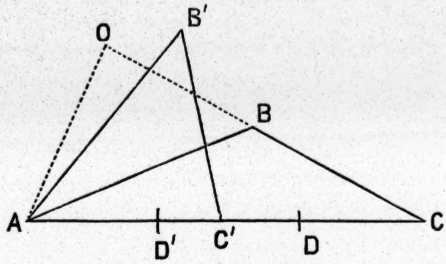
The idea is briefly this,—The explanation of Faraday's rotation of the plane of polarization of light by a transparent diamagnetic requires, as shown by Thomson, molecular rotation of the luminiferous medium. The plane polarized ray is broken up, while in the medium, into its circularly-polarized components, one of which rotates with the ether so as to have its period accelerated, the other against it in a retarded period. Now, suppose the medium to absorb one definite wave-length only, then—if the absorption is not interfered with by the magnetic action—the portion absorbed in one ray will be of a shorter, in the other of a longer, period than if there had been no magnetic force; and thus, what was originally a single dark absorption line might become a double line, the components being less dark than the single one.

Other allied forms of experiment connected with this subject were discussed.

### (*b*) On a Mechanism for Integrating the General Linear Differential Equation of the Second Order.

I am anxious to explain to the Society a kinematical device for the solution of the *General Linear Differential Equation of the Second Order* before I become acquainted with the principle of the integrating machine which, I understand, was described last Thursday by our President to the Royal Society.

My arrangement consists of a combination of two equal modifications of Ammsler's Planimeter, ABC, AB′C′, the wheels of which are attached at the joints B, B′. C′ slides along AC, and the length of AC can be altered by turning either of the heads D, D′, of coaxal screws of equal pitch. Now, if we suppose D connected with the




wheel at B, and D′ with that at B′, by means of universal flexure joints (Thomson & Tait's "Natural Philosophy," § 109), it is obvious that the length of AC will depend upon its angular position, and upon the motion of C′ along AC.

Let AB = AB′ = $a$, BC = B′C′ = $b$, AC = $r$, AC′ = $r_1$, $\angle$ ABC = $\phi$, and let $\theta$ denote the position of AC. Then, if the whole turn through an angle $d\theta$, the motion of B perpendicular to CB is the same as if it had rotated about O, where $\angle$ AOB is a right angle. Hence, if $\rho$ be the radius of the wheel at B, $d\psi$ the angle through which it rotates,

$$\rho\, d\psi = -a \cos\phi\, d\theta = \frac{r^2 - a^2 - b^2}{2b}\, d\theta .$$

A similar expression holds, of course, for B′. Now, if $\alpha$ be the inclination of the threads of the screws, one right, the other left, handed,

$$dr = \rho\,(d\psi - d\psi')\tan\alpha ,$$

$$= \frac{\tan\alpha}{2\,b}(r^2 - r_1{}^2)\, d\theta .$$

Now C′ may be made to move along any curve we choose, so that $r_1$ may be any assigned function of $\theta$. Hence, by introducing the constant factor $\frac{\tan\alpha}{2b}$ for $r$, we may give the equation the form

$$\frac{dr}{d\theta} = r^2 - \Theta$$

to which the solution of the general linear differential equation of the second order can always be reduced.

(*c*) The Electric Conductivity of Nickel. By C. Michie Smith and J. Gordon MacGregor.

Pure nickel foil, obtained in Paris by Dr Andrews, was cut into a spiral about 20 inches long, and it was on this spiral that all the following experiments were made. During the month of November 1875 a large number of experiments were made as to its thermo-electric properties, and these were found to be almost identical with that of the specimen from observations on which the line was laid down on the "thermo-electric diagram." (*Trans. R.S.E.*, 1872-3.) This line, it will be remembered, is a peculiar one, and is very similar to that of iron, with this difference, that the peculiar changes take place at much lower temperatures in nickel than in iron. Having thus finally determined the position of the line in the thermo-electric diagram, we were anxious to discover whether, like iron, it exhibited other peculiarities about the same temperature, and for this end we made the following experiments on the electric conductivity at different temperatures. The method of observation was as follows:—

To the two ends of the nickel spiral stout copper wires were soldered, and the whole was carefully fastened together in such a way that no two coils of the spiral could touch each other. Side by side with this nickel spiral was placed a similar spiral of soft platinum wire of approximately equal resistance. This platinum was part of a wire the electric conductivity of which had been formerly carefully tested, and had been found to obey very strictly the law of being proportional to the absolute temperature. These spirals were then placed in a large pot of oil, care being taken that they hung quite free from the sides of the pot, and the ends of the thick copper wires were led to the pools of a mercury commutator, so arranged that either the nickel or platinum could be made to form one of the arms of a Wheatstone's bridge, in connection with a very delicate Thomson's dead-beat mirror galvanometer. In making the observations the oil was heated by a powerful Bunsen burner, and constantly stirred. By this means it was found perfectly practicable to keep the oil sensibly at the same temperature during the time necessary to find the resistance of the two wires

by the ordinary balance method. That no errors were caused by thermo-electric effects was proved repeatedly during the experiments by completing the circuit without the galvanic cell, when no current was shown on the galvanometer. The results obtained for the nickel entirely agreed with what had been anticipated from the thermo-electric properties. For, when the conductivity is plotted in terms of the temperature, the curve shows a sudden change in direction at a temperature of about 149° C. (300° F.), indicating that there is at that point a sudden change in the rate of alteration of the conductivity with change of temperature. The curve obtained for nickel can be very well represented by two straight lines inclined to each other at an angle of about 9°, while the curve got for the platinum wire is strictly a straight line.

That no part of the effect was due to the conductivity of the oil was amply proved by the following experiment:—

Two pieces of platinum foil, each having a surface of 2·5 square inches, fastened to the ends of copper wires, were plunged in the oil when it was at a temperature of 550° F., and were kept a quarter of an inch apart; the resistance of the oil between them was then measured, and was found to exceed 9 megohms, while the resistance on causing them to touch fell to a small fraction of an ohm.

After a series of experiments had been made with the nickel, the whole spiral was heated to a white heat in the flame of a Bunsen burner, and allowed to cool in the air; another series of experiments was then made on the conductivity, but no change was observed.

The following tables contain the observations for two of the experiments, side by side with the values of the conductivity, calculated on the supposition that the curves are best represented by straight lines—the platinum being represented by a single straight line, while the nickel is represented by a broken line. The calculated and observed values, it will be seen, agree very closely with each other, except where a divergence is to be expected, namely, at the intersection of the two lines (nickel). The equations were taken from the lines obtained by plotting the conductivity in terms of the temperature. R is the resistance in thousandths of an ohm, t the temperature in degrees F.:—

January 14th, 1876.

| Temperature Fahrenheit. | Nickel. Formula up to t=272°. R.=·525 t + 131. Resistance = $\frac{\text{ohms}}{1000}$. Observed. | Calculated. | Difference. + | Difference. − | Platinum. Formula R = ·34 t + 176. Resistance = $\frac{\text{ohms}}{1000}$. Observed. | Calculated. | Difference. + | Difference. − |
|---|---|---|---|---|---|---|---|---|
| 53° | 159 | 159 | ... | ... | 192 | 194 | ... | 2 |
| 69 | 168 | 167 | 1 | ... | 200 | 199 | 1 | ... |
| 99 | 183 | 183 | ... | ... | 210 | 210 | ... | ... |
| 132 | 200 | 200 | ... | ... | 221 | 221 | ... | ... |
| 162 | 215 | 216 | ... | 1 | 230 | 231 | ... | 1 |
| 194 | 233 | 233 | ... | ... | 242 | 242 | ... | ... |
| 218 | 247 | 245 | 2 | ... | 250 | 250 | ... | ... |
| 247 | 265 | 261 | 4 | ... | 260 | 260 | ... | ... |
| | Formula above t = 272°. R = ·775 t + 64. | | | | | | | |
| 279 | 285 | 280 | 5 | ... | 271 | 271 | ... | ... |
| 307 | 305 | 302 | 3 | ... | 281 | 280 | 1 | ... |
| 345 | 331 | 331 | ... | ... | 293 | 293 | ... | ... |
| 376 | 354 | 355 | ... | 1 | 303 | 304 | ... | 1 |
| 440 | 401 | 405 | ... | 4 | 323 | 326 | ... | 3 |

January 18th, 1876.

Resistance = ohms ÷ 1000.

| Temperature Fahrenheit. | Nickel. Formula up to t = 306°. R = ·58 t + 133. Observed. | Calculated. | Difference. + | Difference. − | Platinum. Formula R = ·326 t + 187. Observed. | Calculated. | Difference. + | Difference. − |
|---|---|---|---|---|---|---|---|---|
| 102° | 194 | 192 | 2 | ... | 219 | 220 | ... | 1 |
| 136 | 212 | 212 | ... | ... | 230 | 231 | ... | 1 |
| 169 | 230 | 231 | ... | 1 | 242 | 242 | ... | ... |
| 229 | 266 | 266 | ... | ... | 262 | 262 | ... | ... |
| 301 | 312 | 308 | 4 | ... | 285 | 285 | ... | ... |
| | Formula above t = 306°. R = ·775 t + 74. | | | | | | | |
| 363 | 356 | 355 | 1 | ... | 305 | 305 | ... | ... |
| 407 | 386 | 389 | ... | 3 | 319 | 320 | ... | 1 |
| 447 | 420 | 420 | ... | ... | 333 | 333 | ... | ... |
| 502 | 466 | 463 | 3 | ... | 348 | 351 | ... | 3 |

An attempt was made to discover whether or not the conductivity curve had another peculiar point corresponding to that in the

thermo-electric curve at a high temperature. For this an arrangement was used similar to that employed for the iron wire in the experiments formerly described ("Proc. R. S. E." 1874–75, pp. 629–631). But no results were obtained, owing to the breaking of the nickel ribbon when exposed to the great heat of the white hot cylinder.

The following Gentlemen were elected Fellows of the Society:—

William Skinner, Esq.
J. Ballantyne Hannay, Esq.
Peter Denny, Esq.

*Monday, 21st February* 1876.

Sir WILLIAM THOMSON, President, in the Chair.

The following communications were read:—

1. On the Structure of the Body-wall in the Spionidæ. By W. C. M'Intosh.

In regard to external form, *Nerine foliosa*, Sars, is generally taken as the type of the family, and therefore it may be selected for structural examination in the first instance. Anteriorly the pointed snout is completed by the intricate interlacing of the muscular fibres beneath specially thickened cuticular and hypodermic layers. As soon as the body-wall assumes a rounded form, a layer of circular and oblique muscular fibres occurs beneath the hypoderm, the majority having the latter (*i.e.*, the oblique) direction. In the centre of the area the œsophagus is suspended by strong muscular bundles (the most conspicuous of which are vertical) passing from the hypodermic basement-layer in the middle line superiorly to be attached to the œsophagal wall. A second series, as they descend to their insertion at the ventral surface, give lateral support to the tube; while a third group interlace in a complex manner, and, with the blood-vessels, fill up the space between the œsophagus and the wall of the body.

Toward the posterior part of the head is found—on the dorsal surface—a slight hypodermic prominence, which indicates the position of the central ganglia of the nervous system; the latter

being quite external to all the muscular layers, and covered only by the cuticle and hypoderm. In a line with the first bristles, the layers have assumed a more definite appearance. Beneath the hypoderm is a circular muscular coat, which, however, is somewhat irregular in its arrangement; for, toward the dorsal region, the layer spreads out at each side, and the fibres mix with the oblique muscles of that part, while only a very thin layer stretches across the middle line of the dorsum. Within the former is a more or less developed longitudinal layer—best marked at the ventral aspect. A long oblique muscle extends from the lateral dorsal region on each side to the middle of the body-wall; and an important feature is the situation of the nerve-cord in close proximity to the inferior attachment of this muscle to the hypodermic basement-layer. Various muscular fasciculi, as before, attach the œsophagus to the body-wall, and the bristle-muscles and those of the lateral appendages have made their appearance.

A little behind the foregoing it is noticed that the circular muscular layer is less continuous (though strong inferiorly), and that the longitudinal has been grouped by the other fibres into certain definite bands, the most conspicuous being a double dorsal and two lateral. The former fibres, indeed, have now assumed considerable bulk, a thin circular layer only intervening between them and the hypoderm. The formation of the lateral longitudinal muscles, again, is interesting on account of their homological bearings. From the inferior bristle-tuft, and from the region on each side of it, a strong series of muscular fibres converges toward the side of the œsophagus, and then splits into two bands. The outer bundle is the more powerful, and at the infero-lateral region of the body it bends somewhat sharply outward to be attached to the wall. The fibres thus arch over a chamber on each side for the lodgment of the ventral longitudinal muscle. In ordinary transverse sections they are much stronger than the other, and, moreover, have the nerve-cords at their insertion. The second series slants downward and inward, and is chiefly composed of fibres passing from the dorsal arch by the side of the œsophagus to mingle with the circular fibres at the ventral surface. A thin layer of longitudinal fibres also occurs on the internal aspect of the ventral transverse band (a part of the circular coat).

As we proceed backward, the lateral longitudinal muscles gradually increase in breadth, while the great oblique bands nearly meet in the central line inferiorly. The dorsal longitudinal fibres are grouped in two symmetrical masses, and a strong band passes between the edges of the ventral longitudinal muscles,—the median longitudinal fibres formerly indicated lying immediately within this layer. The nerve-cords have now descended quite to the ventral surface, and have a pale intermediate area.

As soon as the body assumes a transversely-elongated form, the dorsal longitudinal muscles become much extended, and are, besides, intersected by the powerful vertical bands, which sweep from the dorsal basement-layer to the ventral surface, through the lateral longitudinal muscles (now for the most part ventral in position). The oblique muscle on each side is more horizontal, passing from the inferior bristle-bundle to the median line at the ventral surface, and going right through the vertical bands before insertion. The nerve-cords lie close together below the transverse muscle, and a small neural canal exists at the inner and upper border of each. There are still a few longitudinal fibres between the ventral attachments of the oblique muscles. The alimentary canal shows internal circular and external longitudinal fibres.

It is very soon apparent, in proceeding backward, that the vertical muscles descending from the dorsal to the ventral surface do not interdigitate with the great longitudinal muscles throughout their whole extent. They leave, as observed by the lamented M. Claparède, at the external border of each dorsal muscle a considerable mass, which bends downward, and presents in transverse section a distinctly pennate appearance. A similar arrangement occurs at the outer and inner extremities of the ventral longitudinal muscles. Finally, the nerve-cords now have a single and very large intermediate neural canal. The foregoing condition continues with little modification to the tip of the tail; though the dorsal pennate process disappears, the muscle itself being separated from its fellow, and considerably diminished in bulk, while the transverse fibres between it and the hypoderm have greatly increased.*

* The late M. Claparède, in his "Structure des Annél, Sédentaires," p. 15, &c., pl. xv., gives the structure of the hypoderm, and notices the pennate

In *Scolecolepis vulgaris*, Johnst., the body-wall is similarly constructed. Anteriorly the central ganglia of the nervous system lie outside the muscles on the dorsum, and the cords rapidly pass downward to the inferior attachments of the oblique muscles. In this region there is also a dense mass of longitudinal muscles. As soon as the oral aperture is completed posteriorly by the frilled hypoderm, the following arrangement occurs:—Within the hypoderm is an irregular circular coat, the most conspicuous part being a broad belt, which bounds the mouth at the ventral border, and stretches between the great longitudinal muscular masses on each side. Superiorly a short but distinct band also appears under the central hypodermic elevation. A fasciculated longitudinal muscle (dorsal) lies below the latter on each side, its inferior surface being attached to a chitinous sinuous band, which forms a space by its upward curve from a raphe. A somewhat triangular interval occurs, moreover, between the muscles in the median line. The form of this chitinous arch is maintained by strong transverse fibres, which curve from raphe to raphe. At the latter, on each side, there is almost a rosette of muscular fibres, the chief fasciculi being directed downward and outward in transverse section. Outside the foregoing dorsally are various oblique bands, the superior stretching from the dorsum downward and outward to the lateral hypoderm, while the lateral pass downward and inward. The chitinous arch gradually disappears as the dorsal muscle becomes fully developed. Behind the preceding region the arrangement consists, as in *N. foliosa*, of a double dorsal and two lateral longitudinal muscles, with the vertical and oblique bands, the latter passing through the former near the ventral attachment. There is also a very strong transverse ventral muscle, with a series of longitudinal fibres internally. Each nerve-cord in transverse section presents a distinct though small neural canal. The hypoderm, as well as the muscles, seems to be more largely developed than in *N. foliosa*, a feature corresponding with the increased size of the nerves.

After the dorsal muscles have expanded into a broad layer, the same interlacing with the vertical bands occurs, but the pennate

muscles and the arrangement of the nerve-cords of this form, but the foregoing observations do not interfere with his remarks.

arrangement formerly noticed does not appear in this form. The two neural canals soon increase in size, and approach each other in the middle line. With the exception of the great development of the ventral longitudinal muscles posteriorly, little further change takes place in the structure of the body-wall.

The situation of the central ganglia in *Scolecolepis cirrata*, Sars., corresponds with the preceding, and the nerve-cords follow the progress of the oblique muscles toward the ventral surface, each trunk having a small neural canal. When the body wall is completely formed (for instance about $\frac{1}{3}$ in. from the snout), the great size of the longitudinal muscles is conspicuous. The dorsal form a thick superior arch, and proceed a considerable distance down the lateral wall; while the ventral muscles constitute two great curved masses in transverse section, the inner border of each being so carried upward that a deep ventral sulcus is formed for the nerve-trunks and their hypodermic external investment. For the same reason the strong oblique muscles are rendered nearly horizontal. The rounded firm nature of the alimentary canal gives little scope for the development of vertical fibres.

The structure of the body-wall in *Spio*, and the position of the ganglia and nerve-trunks correspond with the foregoing in general features. The same may be said of the rarer *Prionospio*, which has two neural canals inferiorly.

In *Polydora ciliata*, Johnst., the body-wall anteriorly is characterised by the great development and bifid nature of the median ridge, which is flanked on each side by a prominent process of the hypoderm. In transverse section, the snout a little behind the tip presents, on each side of the dorsal process, a large rounded lobe, which projects downward to the oral aperture. Externally, the lobe is composed of a thick layer of hypoderm, having internally a series of circular fibres, which come from the transverse dorsal arch in the form of a loop on each side. The fibres pass downward within the hypoderm, curve inward ventrally, and then proceed upward over the œsophagus to the point of commencement. A well-marked series of longitudinal fibres lines the outer division of the loop, and afterwards merges into the ventral longitudinal muscle of the side. The dorsal arch of transverse fibres cuts off the hypodermic process, containing the nerve-ganglia, and in the

cavity which forms therein is superiorly a small group of longitudinal fibres.

Behind the foregoing a transverse dorsal layer is found beneath the central and now solid hypodermic process, next the dorsal longitudinal muscles (the fasciculi of which, in the middle line, are directed downward and outward, while the outer are directed downward and inward); then a kind of X shaped process occurs in the centre, the legs of the X being prolonged horizontally, so that the whole resembles a figure of ∞, the two spaces containing muscular fasciculi. The lateral and oblique muscles are largely developed, the latter having the nerve-cord on each side below its ventral attachment.

The most interesting point in this form, perhaps, is the structure of the fifth body-segment, which bears the remarkable hooks characteristic of the genus, besides peculiar bristles with spear-shaped heads, and a minute fascicle of the ordinary structure. Immediately in front of the hooks, the body in transverse section shows externally a circular coat, which is thin at some parts, but greatly developed at others. Dorsally a very powerful series of fibres spread outward from the middle line on each side—some becoming continuous with the circular coat, others passing obliquely outward and downward to the superior bristle-bundle. Inferiorly a strong transverse band lies over the nerve-trunks, and forms an external investment to the ventral longitudinal muscles. The oblique muscle comes from the lower bristle-bundle, and joins the former over the nerve-trunk, after piercing the vertical bands. The superior longitudinal muscle forms a great mass on each side, and it interdigitates with the fibres of the vertical muscle. The latter is greatly developed, especially at its inner border, next the œsophagus. The same (vertical) fibres pierce the ventral longitudinal muscle in the compartment formed for it by the circular and oblique bands. The size of the ventral is less than that of the dorsal longitudinal muscles. A somewhat strong group of longitudinal fibres lies within the ventral transverse band. Finally, each fascicle of the ordinary bristles has a V shaped series of fibres extending from the base of the tuft to the lateral wall, and interdigitating with those from the transverse and other muscles of the region.

As soon as the powerful hooks of the fifth segment appear, the entire area—from the alimentary canal to the body wall—is occupied by their muscular apparatus. This consists of a dense series of fibres, which slant from the matrix of the bristles superiorly upward and outward to decussate with the fibres at the upper and outer angle of the body-wall. A still stronger series of fibres occur in the inferior division; the inner are nearly vertical, the rest incline downward and outward. It would appear, therefore, that this powerful muscular mass chiefly acts on the hooks, so as to bring their curved points against the wall of the tube or tunnel. The strong inferior fibres likewise gain additional purchase by passing through the ventral longitudinal layer to be attached to the basement-tissue of the hypoderm. In this region the nerve-cords form two almond-shaped bodies in transverse section in the ventral hypoderm, and they are separated by a distinct interval.

Posteriorly the nerve-cords still remain separate, and a large neural canal lies between them. A well-marked pennate process of the ventral longitudinal muscle occurs at its inner (median) edge.

The foregoing forms, in conclusion, were compared with the structure of *Mœa mirabilis*, Johnst., an aberrant member of the Spionidæ.

## 2. Note on Circular Crystals. By E. W. Dallas.

At long intervals notices of circular crystals have appeared before this Society. In 1853 Sir David Brewster read a paper on the subject, which followed one by Mr Fox Talbot in 1836, and which again had been preceded by one from Sir David Brewster about twenty years before. It is not easy to account for these long intervals, unless they may be attributed to difficulty and uncertainty in manipulation, for except in very few instances the crystals observed by Sir David Brewster are of microscopic size, and, he remarks, require the perfection of optical appliances for their observation, and naturally so when crystals of the 200th of an inch in diameter are looked upon as of respectable size.

Some time ago, being occupied with the subject, I found that by impeding crystallisation by means of gum arabic, circular crystals were formed of a greater size.* This took place with certain salts,

* Perfect crystals were exhibited up to two inches in diameter.

but not with all that were tried. Among the successful instances may be mentioned sulphate of copper, binacetate of lead, muriate of morphia, and other similar salts, which afford beautiful crystals, and are very easily manipulated. The method of proceeding is similar in all; for instance, to a solution of sulphate of copper, let gum arabic or common dextrine be slowly added until it pours oily, then tried, and more gum or more salt added until the result is satisfactory. No precise general rule can be given, as each salt will be found different in the quantity of gum required. The solution is then to be spread thinly over a plate of glass, and dried rapidly before a clear fire, or, if the plate is small, it may be dried over a gas-burner. Upon cooling, the plate, if left to itself, soon shows numerous small specks, which will gradually develope themselves into circular crystals. The process is hastened and a better result obtained by breathing on the plate, when, after a short time, they may be observed to start out very beautifully. These crystals while growing are extremely sensitive, any variation in the moisture applied for their formation resulting in the production of rings.

The growth of the crystals once begun is extremely regular. When the centre is of inappreciable size they are circular, and they proceed onwards in that form until stopped by other crystals, or until the whole vacant space is occupied by them. Should the origin have a definite shape, then that is carried on by the crystals arranging themselves always perpendicularly to the outline of that origin, while, should it be a straight line, they form beautiful fringes perpendicular to the line, and terminate at each extremity in semicircles.

The centres round which the crystals arrange themselves may be either some foreign body in the film, or be determined by some molecular arrangement of the salt at a particular point in the film. They seem to originate spontaneously, and subject to no apparent rule, for foreign particles, and even minute crystals, will not always determine centres; in fact a film may be full of little crystals of the salt, and to very few of them can the circular arrangement be traced.

The crystals present themselves under two aspects in all the salts that I have examined,—a true and an abnormal form. I designate the true form as that in which the crystallisation proceeds by the formation of spicular crystals radiating from a centre and

in optical contact, while in the other many different forms may be observed, in which the component crystals are more or less of a laminate structure, often presenting most beautiful appearances. I have made this distinction, having observed that in some cases at least the true crystals are permanent, while the laminate are not so under like conditions, but change into the true form by time, or may become altogether disintegrated in a damp atmosphere into a confused mass, having few optical properties. The crystals also possess distinctive optical properties.

The production of either of these classes of crystals appears to depend on two conditions, namely, on the thickness of the film, and on the amount of moisture applied in their production,—a thin film and a moderate supply of watery vapour inducing the true form, while a thick film and an increased quantity so alter the structure that at last, although the crystallisation may proceed from a centre, the circular character is entirely lost.

Spherical crystallisation, to which the circular is to be referred, is very frequent in mineral substances. It may be seen also in the well-known experiment of the rapid crystallisation of a supersaturated solution of acetate of soda, by the introduction of a centre round which the salt forms a spherical mass, and the surface, when the action has ended, presents all the appearance of a circular crystal.

However difficult it may be to account for the origin of crystals, their growth in a circular form, when once the centre is determined in a film, is very obvious in those cases where they are produced. In the preparation of the film, not only is the superfluous solvent rapidly evaporated by heat, but a considerable part of the water of crystallisation is also driven off, and there is left on the glass plate a film of an amorphous substance, which, either by attracting moisture spontaneously from the atmosphere, or by having it added, allows the salt, whatever it may be, to resume its crystalline form. That this is the case may be seen from the fact that the crystallisation will take place, and that in a circular form, if the drying of a plate is stopped just at that point when there is sufficient water left to enable the crystals to form when the plate is cooled. In this case their formation is a repetition of the acetate of soda experiment already alluded to. A plate may sometimes also be dried and crystallised, and on being again exposed to heat the crystals

will disappear, and the plate may be re-crystallised, but not so well as at first, and not from the same centres. This is the case with the binacetate of lead.

There is one remarkable form of these crystals which is of frequent occurrence, and which Sir D. Brewster seems to have observed only in mannite. The form looks very extraordinary, a properly prepared plate presenting the appearance of being covered with paraboloids. These are simply circular crystals which have been formed in the film while the general crystallisation has proceeded from one side, and is caused by the crystals overtaking one another in their onward progress in one direction. Taking this case in its simplest form by supposing equal rates of crystallisation proceeding from the edge of a plate and from a centre near the edge, their line of contact will necessarily be a parabola, for it is evident the edge is a directrix and the centre the focus of such a figure, but this will very seldom occur, since the growths are not only generally unequal, but also vary in themselves.

The question may be put whether gum arabic is the substance best suited for these experiments. I have not tried many, and with those that I have experimented upon the results were not so satisfactory. That other agents may be employed, according to the salts or other substances to be treated, is certain; for example, collodion, as may be seen in a coated photographic plate that has been allowed to dry after excitation in the nitrate of silver bath, when circular crystals of iodo-nitrate of silver are often produced. This salt, be it observed, although produced through the agency of water, being at once decomposed by that element.

Having confined my observations mainly to a very small number of salts, it would be premature to offer any general conclusion on the structure of their circular crystals or on their optical properties; besides, from the great interest Professor Tait has shown in the subject, I am in great hopes that he may be induced to make some investigation in it. I shall only mention one point that I have observed in the effect of some crystals on the black cross, something in their structure producing a more or less spiral arrangement of the arms in a horizontal direction, while on one occasion a vertical arrangement was observed, in which the arms seemed to be raised one above the other like four quadrant steps. This effect I have only

seen once in a crystal of sulphate of magnesia, and have not been able to reproduce anything of the kind, but the crystallisation of that salt is so varied and irregular that many things may pass unobserved.

The only other point I shall lay before the Society is, that I have succeeded in producing a crystallisation very similar to that of frost-pictures on a window pane, and I hope to be able to make the imitation more perfect. For this purpose I have employed the sulphate of copper and magnesia,—a salt that crystallises under the rhombohedral system, the same with that of ice. This salt crystallises in the films from centres in a most remarkable manner in four different modes, viz., the true circular, the laminate, a branched or dendritic form, and another that I hardly know how to designate, unless it may be called the ostrich plume form. All these different forms may be observed on the plates, either simply or in combination, and produce most varied and singularly beautiful effects.

### 3. Preliminary Note on the Flame produced by putting Common Salt into a fire. By C. M. Smith, Esq. (Communicated by Professor Tait).